SCIENCE

FRIDAY, MARCH 10, 1911

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE INFLUENCE OF ASTRONOMY ON MATHEMATICS

THERE are probably many reasons why the members of the eleven sections of the American Association, representing at least fifteen sciences, have united in a single society. One of these is undoubtedly that the mingling of men of varied chief interests, points of view and methods of work has at least a tendency to correct those intellectual provincialisms which are characteristic of isolation, and to show how wide and how rich is the field of scientific activity. While it is unquestionably advantageous on some occasions for narrower groups of men whose interests are more nearly common and whose scientific activities run more nearly in the same channel, to meet apart for the consideration of their own special problems, yet on the whole the benefits to be derived from occasional joint meetings are so great that it is earnestly hoped the American Association will prosper in the future even more than it has prospered in the past, and that the individual scientific societies will not cease to cooperate with it.

If it is agreed that there are real benefits to be derived from an association of many distinct scientific societies, it will equally be granted that some advantages may be obtained from a meeting where so many points of view, modes of thought, and methods of investigation are represented as among the members of Section A. These diverse, and in some cases conflicting, points of view have arisen from the narrow specialization of recent times, and from the fact that the bounds of our knowledge have extended more rapidly

than our individual capacities for encompassing them. Historically, astronomy and mathematics have been most intimately related. In antiquity the roll of the celebrated masters in one of these sciences would almost exactly coincide with that in the other; and in more modern times the names of Newton, Euler, Lagrange, Laplace, Gauss, Cauchy, Poincaré and many others, which are equally honored by both astronomers and mathematicians, show how much they still have in common. It is for this reason that in accepting our secretary's invitation to present a paper before you I have chosen to lay emphasis on these close relations; and in doing so I apologize only because I am so poorly prepared adequately to treat so worthy a theme. If you will permit the interjection of a purely personal remark, I should like to state that it is not solely because of the close relations of astronomy and mathematics that I have chosen my topic, but because a most fortunate experience has taught me that the lack of sympathy, if not of respect, for the efforts in one domain by workers in another is due almost wholly to a lack of acquaintance with them. I refer to the fact that for fifteen years I have been intimately associated with one of the former presidents of the American Association, with the present president, and with the vice-president of Section A. The first has taught me how the mind of the naturalist works, how vivid and constructive is his imagination, how fertile he is in inventing hypotheses, how exhaustively he gathers his data, and how impartially he weighs evidence where prejudices easily might have influence; the second has shown me how keen are the intuitions of the physicist, how his quantitative estimates almost rival in accuracy mathematical calculations, and how his marvelous instruments increase the delicacy of our senses a million fold at a single leap, making it unnecessary for us to wait inconceivable ages for their evolution to that degree of perfection; the third has revealed to me how beautiful are the logical structures which can be built on independent and consistent hypotheses, how keen are the pleasures in establishing all the harmonious relations involved in a mathematical theory, and how great is the satisfaction in the discovery of that which is common and fundamental to many apparently distinct theories. Here I confess my profound and equal respect for all these phases of scientific activity, and my belief that they are all of fundamental importance. If in my attempts to accomplish something I follow more nearly along the line of one than another, it is because I believe that on account of personal tastes and training I am less likely to fail there than in the others.

There does not seem to be a disposition on the part of any one to limit the field of activity of the astronomer. He is supposed not only to know how to measure the distances, to calculate the motions, and to determine the composition of the heavenly bodies, but also to understand fully those complex factors which produce the weather changes, to be familiar with certain mysterious forces which bring good or bad luck to an individual, to have reliable data respecting the location of heaven and its antithesis, and to be an expert on the questions of the freedom of the will, the existence of an infinite being, and the immortality of the soul. But the matter is quite different in the case of the mathematician. Often he is criticized for busying himself with pure fictions of the mind rather than with the so-called actualities of physical problems. It is no mere passive antagonism, for there are many places in these halls to-day where a storm can be raised

by starting a discussion of imaginary numbers, hyperspace, or the non-euclidean geometries. It is easy to find men who will mark out the regions within which mathematicians should exercise their powers. It is an interesting psychological phenomenon that a specialist who has spent many years on a subject and has become a recognized authority in it seldom, if ever, will make any definite and general statement in regard to it; yet often he will not hesitate to make sweeping dogmatic assertions respecting things entirely outside his line, for example (to use a harmless illustration), respecting the merits of the tariff or the crime of seventy-three. Even those who have been expert in mathematics have differed much among themselves respecting what should be its highest aims. Fourier, in reporting on the work of Jacobi to the Academy of Sciences, said that natural philosophy should be the principal object of the meditations of mathematicians. In the introduction to his theory of heat referring to analysis he wrote "there could not be a language more universal and more simple, more exempt from errors and obscurities, that is to say, more worthy of expressing the invariable relations of natural objects. Considered from this point of view, it is coextensive with nature itself; it defines all the sensible relations, measures the times, the spaces, the forces, the temperatures; this difficult science is formed slowly, but it retains all the principles it has once acquired. It grows and becomes more certain without limit in the midst of so many errors of the human mind." Replying to the reproach of Fourier, Jacobi, in a letter to Legendre, said: "It is true that M. Fourier had the opinion that the principal end of mathematics was the public utility and the explanation of natural phenomena; but such a philosopher as he is should have known that the unique end of science is the honor of the human mind, and that from this point of view a question of number is as important as a question of the system of the world." Gauss agreed, for he said that mathematics is the queen of the sciences, and that arithmetic is the queen of mathematics.

Obviously it is just that the astronomer should allow the mathematician all the latitude in defining the limits of mathematics that he himself would desire if he were permitted to mark out the borders of the field of astronomy. It would be considered unwarrantable interference and an evidence of hopeless ignorance if any group of men should attempt to make astronomers confine themselves to those phases of their subject which are immediately useful to a busy world. If astronomy were limited simply to those parts which are necessary for time service on the land and the use of navigators on the sea; if it were necessary to abandon those mathematical theories of the motions of the planets and satellites which are in all respects the most perfect examples in natural science of harmony between theory and observation; if it were no longer permitted to use our powerful instruments in observing the peculiarities of the planets and the sun; if we were compelled to discontinue our investigation as to their origin and evolution; if we were under obligations to give up the spectroscope forever; and if we were forced to forego all further attempts to sound the almost boundless depths of the sidereal system and unravel its mysteries; if astronomy were put under these restraints, I say, then most of those incentives which in all the history of astronomical science have produced the rarest examples of devotion to ideals and the pursuit of knowledge would be removed. Astronomers do not admit the right of a partially informed

world to prescribe the boundaries for their activities, nor do they in turn feel qualified or even inclined to impose any limitations upon the mathematicians. They could not even say what kinds of mathematics will be of use to themselves or to other branches of physical science. To take an example old enough to be understood in correct perspective, the interval between the discovery of the properties of the conic sections by Menæchmus and their first practical use by Kepler was 2,000 years, or more than nine times that which separates us from Newton. Astronomers will admit, then, that if the sole purpose of mathematics were to serve the other sciences, it would not be safe to circumscribe it by any boundaries. And most of them, I think, will go much further and join me in the sentiment that mathematics, altogether apart from its uses in other subjects, has a right to exist; that it is a part of the universe of ideas which to a thinking being is no less real and important than the physical universe; that its proportions and its symmetries which find perfect expression in its wonderful symbolism are, in satisfying the esthetic tastes, on a level with the fine arts; and that the process of drawing its conclusions calls for an exercise of the best and highest faculties we possess. If we were required to describe the proper field of mathematics we might say simply that it includes at least all that which all mathematicians together claim belongs to it.

Having admitted the breadth of mathematics, we have to consider what part of it has had at least its initial inspiration in astronomy. It might, perhaps, be argued with a good deal of justice that all of mathematics has originated directly or indirectly in the experience of the human race; that our capacity for those particular modes of thought which are essential to its development have evolved under the stimulus of the physical world. It is significant, at any rate, that there is such wonderful harmony between the results obtained by mathematical processes and our experiences. But it is not the purpose here to make any such claims, or to become involved in the difficulties of metaphysical discussions. No thesis has been laid down which it is necessary to defend, and no claim that astronomy has had an important influence on mathematics will be filed except where the evidence is perfectly clear and conclusive.

It was noted in the beginning that in ancient times the astronomers were almost invariably also mathematicians and reversely, and consequently that it is difficult to separate the two sciences of that time so as to determine exactly the influence which each had on the other. But there is one case in which the demands of astronomical problems certainly stimulated the development of a mathematical theory. Trigonometry was invented by Hipparchus, who was the most eminent Greek astronomer, both as a practical observer and as a mathematician. He determined the length of the year correctly to within six minutes of its true value, the obliquity of the ecliptic to within five minutes of arc, the annual precession of the equinoxes to within nine seconds of arc, the distance of the moon to within one per cent. of its value, the mean motions of the sun, moon and known planets, the changes in the moon's orbit, he made a catalogue of the fixed stars, etc. There is every reason to believe that these astronomical problems were those in which he was chiefly interested, and they made it necessary for him to develop trigonometry, and especially spherical trigonometry. His work was completed by Gauss nearly 2,000 years

later in connection with the solution of the same problems.

We shall not, however, get any comprehensive view of the relations of astronomy and mathematics by citing, without some classification, isolated examples where the latter is indebted to the former. Such a procedure will give us no idea of the reasons for any of the great movements in mathematical thought. Moreover, the mathematical theories are so interwoven that it is difficult to pick out individual branches and to discuss their origins without being at least very incomplete. Therefore we shall content ourselves with broad classifications of mathematics, and to statements, with illustrations, of those parts where the practical problems of astronomy have had important influences.

Mathematics may first be divided into the metrical and the non-metrical branches. The former are vastly more important than the latter; or, since it is perhaps wise to avoid passing judgment as to what is important, the metrical branches have at least an enormously greater literature than the non-metrical. Recognizing the fact that there is much of a non-metrical character in those subjects which are regarded as being on the whole metrical in nature, and not wishing to insist on the possibility of making an absolute division on this principle, an examination of the Royal Society Index covering publications in mathematics from 1800 to 1900 shows that probably not one part in forty has been devoted to non-metrical mathematics. While there are certain non-metrical aspects of some astronomical problems, it would not be fair to claim that astronomy has had any essential part in inspiring these branches of mathematics.

Now considering only the metrical branches of mathematics, we may divide them into the mathematics of the continuous and the mathematics of the discrete. Here the problem of actually effecting the division is even more difficult than in the preceding case, for there is more intermingling and there seem to be more debatable points. In spite of these difficulties the mode of division is attached to certain fundamental characteristics either of the subject matter or of the processes employed. The ordinary theory of numbers is an example of the mathematics of the discrete. The theory of ordinary equations, for example, linear equations, may be considered as an example of the mathematics of the discrete or the continuous, according as the coefficients are regarded as discrete numbers or continuous functions of certain parameters. In such cases where the ideas of continuity are not essential to the formulation and treatment of the problems, they will be considered as belonging to the mathematics of the discrete. All those branches of mathematics in which continuity is an essential feature, as, for example, those involving derivatives, constitute the mathematics of the continuous.

On the whole, the problems of astronomy have not given rise to the mathematics of the discrete. While the physical universe seems to be made out of discrete things—atoms, corpuscles, units of electricity—it changes continuously from one state to another. Since a large part of the problems of the natural sciences relate to changes of position or state, such as the motion of a world or the evolution of an animal, this continuity is forced into the foreground in the applications of mathematics to physical questions. Consequently, in seeking for places where astronomy has made real contributions to mathematical theory we may restrict our search to the mathematics of the continuous. If there are no other subjects which have made similar contributions, we have at once the

answer to the question of the extent of its influences. Since astronomy is more thousands of years old than most of the other natural sciences are centuries, it has naturally called forth most of those mathematical processes which have been needed in the others. About the only other natural science which has given rise to important mathematical theories is physics, which has forced attention to certain classes of partial differential equations and to the statistical methods employed in the kinetic theory of gases. Another important advantage astronomy has enjoyed is the delicate character of many of its observations and the high degree of precision of many of its theories. These have naturally directed attention to the questions of logical rigor. It is probably known to most of the members of this section that the numerically most perfect theory in all the range of physical science in all time is the lunar theory of our retiring vice-president. But the mathematics of the continuous has not been inspired by astronomy alone, or even by all the physical sciences together. In geometry the questions of tangents and areas have involved the same principles and have given rise to some of the same methods. Consequently we can conclude only that the problems of astronomy have given rise to some of the theories of the mathematics of the continuous.

It will perhaps be worth while to descend for a few minutes from the general to the particular, and to consider more concretely what contributions astronomy has actually made to mathematics. It is agreed by all that the invention of the calculus was one of the most important steps ever made in mathematics. It was founded first by Newton and a little later independently by Leibnitz. The work of either was sufficient to open the way to all that which has followed the invention of this important branch of mathematics. Newton's ideas were largely inspired by the consideration of physical phenomena, as is shown by the terminology and notation he used as well as by the problems to which he applied his methods. He spoke of *fluents* and *fluxions* and used the time as the independent variable, though he knew this was not essential. It simply indicates the stimulus of his ideas. On the other hand, Leibnitz used the terminology of geometry and seemed to have arrived at his ideas of derivatives through the consideration of tangents to curves. These differences constitute an internal evidence of the independence of the work of Newton and of Leibnitz.

The history of the application of the calculus in the century following its discovery constitutes one of the most glorious records of the achievements of the human mind. Mathematicians had a new method of enormous power and the greatest generality, while the laws of motion and the law of gravitation were the keys that unlocked a new universe to them. The work of Clairaut, d'Alembert, Euler, Lagrange and Laplace was one succession of triumphs. With the enthusiasm of explorers they traversed the worlds Newton and Leibnitz had opened, and with Laplace it was supposed the discoveries in them were about exhausted. The point to be emphasized here is that whatever may have been the origin of the calculus, its evolution into the larger domain of analysis in the century following its invention was due almost entirely to the stimulus of physical, and in particular astronomical, problems. There does not seem room for doubt that the very important place which analysis now occupies in mathematics is to a large extent due to its applications to astronomy.

Astronomy not only turned the attention of mathematicians to analysis, but it often determined the precise form their theories

should take. Consider, for example, analytic differential equations. There are five distinct methods of developing their solutions—as power series in the independent variable, as power series in parameters, as limits of equations of finite differences, by successive approximations, and by successive applications of the variation of constants—all of which were devised under the pressure of practical astronomical problems and were applied successfully many years before the conditions of their legitimacy were fully established by mathematical methods. A more recent example is Hill's treatment of the linear differential equation having simply periodic coefficients, the properties of whose solutions were inferred by him from the properties of the motion of the moon. The problems connected with an infinite number of simultaneous homogeneous linear equations also arose in Hill's lunar theory. Poincaré's researches in the problem of three bodies led him to the discovery of many new properties of the solutions of differential equations. The question of the legitimacy of the series used in celestial mechanics, particularly when applied for long intervals, has forced a consideration of the problem of determining what classes of divergent series may be used and what conclusions may be drawn from them; and the same question has stimulated investigations of other modes of representing solutions, particularly as sums of polynomials in the independent variable, having wider domains of validity. In this direction Painlevé has achieved the most important results, and has shown how to construct functions which represent the solution of the general problem of n bodies so long as there are no collisions. If the forces were repulsive instead of attractive the developments would be valid indefinitely. But as Laplace said "nature does not care for analytical difficulties"; in fact, it fills the way of the mathematician with them. As a partial recompense for the difficulties it raises it often suggests methods for overcoming them, and these methods being made general in the symbolism of mathematics constitute new processes often applicable in many other directions.

One of the recent movements in mathematics is in the application of the postulational method. It consists in postulating the existence of certain elements which are wholly without properties except as they are imposed by the postulates and the explicitly stated axioms. The postulates and their implications constitute the theory. It is not to be supposed that the postulates are laid down at random, or even on any simple principle of their individual and separate characteristics. The sole guide is that taken together they shall yield as consequences certain relations which are in advance in the consciousness of the investigator; the additional implications are the contributions which the developed theory makes. I do not know why there has sprung up the recent interest in this method, but it is fundamentally the method used in natural science. The experiences are the certainties given in advance which must be implications of the theories. The atoms, corpuscles, units of electricity, etc., are the postulated elements. The theories are the postulated relations among the elements. If we let $a_1, \cdots, a_n$ represent the experiences, $x_1, \cdots, x_m$ the postulated elements, then we shall have

$$f_i(x_j) = a_i,$$

where the f_i are the theories. If one of these relations fails to hold it is necessary to modify the x_j, or the f_i, or both, and it is easy to cite examples from the history of science illustrating all these possibilities. The recognition of the fundamental iden-

tity of the process of constructing theories in the realms of natural science and of developing mathematics by the postulational method will undoubtedly be of great value to the former in showing what is really essential, and to the latter in inspiring almost endless points of view.

It is not necessary to cite more examples to show that mathematics owes much to astronomy, especially in the field of analysis. If it were proper to strike a balance it could probably be shown that the debt has been more than repaid, but in these unselfish sciences the privileges of foreign service are cherished as much as the treasures of domestic achievements, and therefore we content ourselves with the recognition of the interrelations.

In closing I may point out the truism that these interrelations are not limited to astronomy and mathematics. It is to the glory of astronomy that in it were initiated the two most fundamental intellectual movements in the history of mankind, viz., the establishment of the possibility of science and of the doctrine of evolution. Our intellectual ancestors in the valleys of the Euphrates and the Nile and on the hills of Greece looked up into the sky at night and saw order there and not chaos. By painstaking observations and calculations they discovered the relatively simple laws of the motions of the heavenly bodies, whose invariable and exact fufilment led to the belief that the whole universe in all its parts is orderly and that science is possible. In the modern world this conclusion is so commonplace that its immense value is apt to be overlooked, but a study of the superstitions and the hopeless stagnation of those portions of mankind which have not yet made the discovery gives us some measure of its worth. The modern supplement to the conception that the universe is not a chaos is that not only is it an orderly universe at any instant, but that it changes from one state to another in a continuous and orderly fashion. This doctrine that science is extensive in time, as well as in space, is the fundamental element in the theory of evolution and the completion of the conception of science itself. The ideas of evolution in a scientific form were first applied to the relatively simple celestial phenomena. More than a century before the appearance of Darwin's "Origin of Species," and the philosophical writings of Spencer, another Englishman, Thomas Wright, published a book on the origin of worlds. Laplace's nebular hypothesis gave the geologists a basis for their work, which in turn paved the way for that of Darwin. For half a century now the doctrine of evolution has been a fundamental factor in the elaboration of all scientific theories, and its influence has spread to every field of intellectual effort. It has been the good fortune of mankind that his skies have sometimes been free of clouds and that he has been able to observe those relatively simple yet majestic and impersonal celestial phenomena which have not only led to so important results as the founding of science and the doctrine of evolution, but have strongly colored his poetry, philosophy and religion, and have stimulated him to the elaboration of some of his most profound mathematical theories.

F. R. Moulton

University of Chicago

STATISTICS OF GERMAN UNIVERSITIES

The twenty-one German universities show an enrollment for the winter semester of 1910–11 of 54,822 students, as against 52,407 students last winter. During the past five years there has been an increase in registration of no less than 12,432 students. Curiously enough, the winter enrollment exhibits a decrease, although only of a few students, against the previous summer semester, in which

54,845 students were enrolled. The number of women students has grown from 211 five years ago to 2,418 this winter. There has been an increase in the number of students of medicine, philology and history, pure science, and Protestant theology, and a small gain in the number of students of Catholic theology, while there has been a decrease in the number of law students and in the students of dentistry and pharmacy. By faculties and groups the students were distributed as follows: Protestant theology, 2,535 (as against 2,320 in the winter semester of 1909–10); Catholic theology, 1,760 (1,698); law, 10,890 (11,317); medicine, 11,240 (10,135); dentistry, 1,146 (1,395); philosophy, philology and history, 15,525 (14,593); pure science, 7,914 (7,349); pharmacy, 954 (1,279); agriculture, 2,546 (2,085); forestry, 171 (129); veterinary medicine, 141 (107).

So far as the individual universities are concerned, those of Prussia show a larger increase than those of Bavaria and Baden, the ten Prussian universities having enrolled 28,675 students as against 27,244 last year, whereas the three Bavarian universities show an increase from 9,042 last winter to 9,342 this winter, and those of Baden from 4,101 students to 4,254 students; the remaining six German universities have increased their clientele from 11,980 to 12,552 students. The three largest universities (Berlin, München and Leipzig) alone enrolled no less than 39 per cent. of the total German student body, Berlin remaining at the top with an enrollment of 9,686 students, as against 9,242 last winter. This is followed by the University of München with 6,905 students (6,537 last year). The remaining universities range in the following order: Leipzig, 4,900 (4,761); Bonn, 3,846 (3,652); Halle, 2,661 (2,393); Breslau, 2,454 (2,405); Freiburg, 2,246 (2,167); Göttingen, 2,233 (2,230); Strassburg, 2,067 (1,995); Münster, 2,047 (1,906); Heidelberg, 2,008 (1,934); Marburg, 1,981 (1,878); Tübingen, 1,883 (1,760); Jena, 1,637 (1,496); Kiel, 1,439 (1,290); Würzburg, 1,425 (1,424); Königsberg, 1,380 (1,367); Giessen, 1,249 (1,261); Erlangen, 1,011 (1,121); Greifswald, 948 (881), and Rostock, 816 (707).

The figures show that all the universities with the exception of Erlangen, Würzburg and Giessen have increased their attendance, the smallest gains having been made by Göttingen, Königsberg and Breslau, the largest (in percentage) by Halle, Kiel, Jena, Tübingen and Rostock. Since last winter Breslau has been passed by Halle, Göttingen by Freiburg, Heidelberg by Münster, Würzburg and Königsberg by Kiel.

In addition to the 54,822 matriculated students, 3,528 men and 1,772 women are enrolled this winter as auditors, giving a total of 60,122 individuals receiving instruction at the German universities, the largest number in the history of German higher education.

RUDOLF TOMBO, JR.
COLUMBIA UNIVERSITY

MEMORIAL TO CHARLES OTIS WHITMAN

VOLUME 22 of the *Journal of Morphology* will be a Charles Otis Whitman Memorial Volume. This volume was originally intended as a testimonial by former students and colleagues to the founder of the *Journal of Morphology,* Professor Charles Otis Whitman. In view of his untimely death it becomes a memorial volume. In addition to a large number of scientific papers by Professor Whitman's students and associates, illustrated with numerous plates and text figures, it will contain a biographical sketch with portraits of Professor Whitman. The edition of this volume will be sufficient to supply special orders in addition to the regular subscriptions to the *Journal of Morphology.* The contents will be as follows:

"The Sex Cells of *Amia* and *Lepidosteus*," B. M. Allen.

"Male Organs for Sperm-transfer in the Crayfish, *Cambarus affinis*, their Structure and Use," E. A. Andrews.

"Vertebrate Cephalogenesis." 3. "*Amphioxus* and *Bdellostoma*," Howard Ayers.

"On the Bilaterality of the Pigeon's Egg, a Study in Egg Organization," G. W. Bartelmez.

"The Regulatory Process in Organisms," C. M. Child.

"Cell-size and Nuclear Size," E. G. Conklin.

"Oviposition Induced by the Male in Pigeons," Wallace Craig.

"The Life History of the *Scolex polymorphus* of the Woods Hole Region," W. C. Curtis.

"The Transplantation of Ovaries in Chickens," C. B. Davenport.

"On the Regular Seasonal Changes in the Relative Weight of the Central Nervous System of the Leopard Frog," Henry H. Donaldson.

"Spermatogenesis in the Arenicolidæ," E. R. Downing.

"Reproductive Activities of the Squid," Gilman A. Drew.

"Modifications in the Testes of Hybrids from the Guinea and the Common Fowl," M. F. Guyer.

"Some Problems of Cœlenterate Ontogeny," C. W. Hargitt.

"Minimal Size Reduction in Planarians through Successive Regenerations," S. J. Holmes.

"Studies in Fertilization in *Nereis*." 1. "The Cortical Changes in the Egg." 2. "Partial Fertilization," F. R. Lillie.

"The Physiology of Cell-division." 4. "The Action of Pure and Calcium-containing Salt-solutions, followed by Hypertonic Sea-water, on the Unfertilized Eggs of *Arbacia*, with a Theory of the Physics of Certain Features of Mitosis from the Standpoint of the Membrane Theory of Bioelectric Processes," Ralph S. Lillie.

"Anatomical Illustrations before Vesalius," W. A. Locy.

"The Growth and Retrogression of *Corpus luteum* in the Guinea-pig," Leo Loeb.

"The Chemistry of Fertilization," A. P. Mathews.

"The Influence of Inbreeding and Selection on the Fertility and Sex Ratio in *Drosophila*," W. J. Moenkhaus.

"The Spermatogenesis of the *Hempteron Euschistus*," T. H. Montgomery, Jr.

"Further Studies of Ovogenesis and Spermatogenesis in Phylloxerans and Aphids," T. H. Morgan.

"Studies of Variation and Heredity in the Armadillo," H. H. Newman and J. T. Patterson.

"Foot Movements of Molluscs," G. H. Parker.

"The Evolution of the Pearl Organs of American Minnows and Suckers. A Study in the Factors of Descent," Jacob Reighard.

"White and Yellow Yolk in Vertebrate Ova," Oscar Riddle.

"The Structure and Periodicity of the Developing Salpa Chain," W. E. Ritter (With Miss Myrtle Johnson).

"Physiological Animal Geography," V. E. Shelford.

"The Olfactory Organs and the Sense of Smell in Birds," R. M. Strong.

"The Behavior of the Chromosomes in Cross-fertilized Echinoid Eggs," D. H. Tennent.

"Experimental Modification of the Germplasm," W. L. Tower.

"The Ant-colony as an Organism," W. M. Wheeler.

"Studies in Chromosomes." 7. "A Review of the Chromosomes of *Nezara* with some more General Considerations," E. B. Wilson.

"*Paramœcium caudatum* and *Paramœcium aurelia*," L. L. Woodruff.

"Experiments in the Control of Asymmetry in the Development of the Serpulid, *Hydroides dianthus*," Charles Zeleny.

Titles of papers of the following not received: H. McE. Knower, Jacques Loeb, F. P. Mall.

SCIENTIFIC NOTES AND NEWS

THE congress has passed a bill to retire Commander Robert E. Peary, with the rank and pay of a rear-admiral and to extend to him the thanks of congress.

THE vacancy in the board of consulting scientific experts to the secretary of agriculture, caused by the death of Dr. C. A. Herter, has been filled by the appointment of Dr. Theobald Smith, of Harvard University. The appointment was made by the secretary of agriculture, with the full approval of President Taft.

THE Helmholtz medal of the Berlin Academy of Sciences was awarded to Professor van't Hoff shortly before his death.

SIR WILLIAM H. WHITE has been awarded the John Fritz medal for 1911, for "notable achievements in naval architecture," by the board representing the national societies of civil, mining, mechanical and electrical engineering. The first award was made in 1905 to Lord Kelvin, and subsequently to Alexander Graham Bell, Thomas A. Edison, George Westinghouse, Charles Porter and Alfred Noble.

THE gold medal of the British Institution

of Mining and Metallurgy has been awarded to Sir Julius Wernher, in recognition of his services to technological education and in the promotion of the interests of the mining and metallurgical professions.

DR. L. A. BAUER was made an honorary member of the Royal Cornwall Polytechnic Society of England at its recent annual general meeting.

THE Academy of Natural Sciences of Philadelphia has elected Dr. Walter Rothschild, of Tring, England, a correspondent.

PROFESSOR BIER, director of the surgical clinic at Berlin, has been elected a foreign member of the Berlin Academy of Sciences.

MR. NORMAN TAYLOR, editor of *Torreya,* and assistant curator at the New York Botanical Garden, has been appointed curator of plants in the newly established Brooklyn botanic garden.

DR. CLARENCE J. MARSHALL, professor of veterinary medicine at the University of Pennsylvania, has been appointed veterinarian of the state of Pennsylvania.

DAVID ALBERT MOLITOR, professor of topographic and geodetic engineering in the College of Civil Engineering of Cornell University, has resigned from the faculty and returned to active practise.

DR. HENRY C. TAYLOR, professor of agricultural economics at the University of Wisconsin, has been elected associate editor of *The American Economic Review,* published by the American Economic Association. Dr. Taylor will have charge of the subject of agricultural economics.

DR. ALICE HAMILTON, of the Memorial Institute for Infectious Diseases, of Chicago, who investigated the lead industries of Chicago and Illinois with reference to lead poisoning for the Illinois Commission on Occupational Diseases, is undertaking similar work for the federal government.

THE Frederick Sheldon traveling fellowship of Harvard University has been awarded to Latham Clarke, Ph.D., instructor in industrial chemistry.

PROFESSOR GIUSEPPE MERCALLI has been appointed director of the observatory on Mt. Vesuvius, to succeed Professor Matteucci.

PROFESSOR E. B. WILSON lectured on "Heredity and the Cell" before the Society of Sigma Xi of Columbia University on February 23.

DR. L. H. BAILEY, director of the New York State College of Agriculture at Cornell University, delivered a lecture under the auspices of the Pennsylvania Chapter of the Society of the Sigma Xi on February 27, on the subject of "The Country Life Situation."

Two Sigma Xi lectures, one on "Attention" and one on "Types of Mind," were given at the University of Minnesota, on February 9 and 10, by Professor E. B. Titchener, Sage research professor at Cornell University.

THE Columbia chapter of the Phi Lambda Upsilon, the honorary chemical fraternity, was addressed by Dr. H. W. Wiley, of Washington, D. C., on February 18. In his address on the "Relation of Chemistry to the Public Welfare" Dr. Wiley showed the moral influence which a chemist exerts on the community and the position which a chemist will assume in the fight against disease. An informal reception to Dr. Wiley was tendered by the society after the address to welcome him as a member.

THE annual meeting of the Illinois State Academy of Science was held at the University of Chicago on February 17 and 18. Professor J. M. Coulter, head of the department of botany of the university, delivered the presidential address on "The Problems of Plant Breeding."

ON January 21 the Oregon Academy of Sciences met in regular monthly meeting, the address being by W. N. Ferrins, president of Pacific University and one of the Rhodes scholarship committee for the northwest. His subject was "The Rhodes Scholarships and Oxford University." On February 18 Wm. T. Foster, president of the new Reed College, of Portland, spoke on "The American College," giving a brief review of the history of European colleges. At the March meeting Pro-

fessor Albert Sweetser, biologist of the University of Oregon, will deliver an address.

The Society of College Teachers of Education held its convention during the sessions of the Department of Superintendence of the National Education Association on February 23 and 24 in Mobile, Ala. The president of the society was Charles H. Judd, professor of education in the University of Chicago.

Professor Arthur Keith, conservator of the museum of the Royal College of Surgeons of England, gave in February a course of six Hunterian lectures on the fossil remains of man, and their bearing on the origin of modern British types.

After the scientific program, at the session of the Philadelphia College of Physicians, on February 1, Dr. Robert Abbe, New York City, presented to the college the gold watch of Benjamin Rush, and Dr. William W. Keen, on behalf of the donors, presented to the college a portrait of Dr. William Goodell, the gynecologist.

By the instructions of the London County Council, as we learn from *Nature*, a blue tablet of encaustic ware has been affixed to No. 32 Soho Square, W., at one time the residence of Sir Joseph Banks, who was elected president of the Royal Society in 1778 and held that office for forty-one years.

Sir John Murray will give his memorial address on "The Life and Scientific Works of Alexander Agassiz," at Sanders Theater, Harvard University, on Wednesday evening, March 22, at eight o'clock. On account of Sir John Murray's illness this lecture was postponed from February 14.

Dr. Walter Remsen Brinckerhoff, assistant professor of pathology in the Harvard Medical School, the author of important researches on small-pox and leprosy, died in Boston, on the second of March, in the thirty-seventh year of his age.

The death is announced from Berlin of Professor J. H. van't Hoff, eminent for his contributions to physical chemistry.

Dr. Aloysius Oliver Joseph Kelly, assistant professor of medicine in the University of Pennsylvania and professor of pathology in the Woman's Medical College of Pennsylvania, died on February 23, at the age of forty-one years.

The second Central American Expedition of the School of American Archeology reached Guatemala on January 14 and steps were immediately taken to continue the work inaugurated the preceding year. After a preliminary survey of the southern Maya field year (January, 1910), it was decided that the School of American Archeology would undertake the excavation and repair of the ruins of Quirigua in the Department of Izabal, some fifty miles from the Atlantic coast. During the first expedition the ruins were surveyed, and a park laid out surrounding them. The Great Plaza was cleared of underbrush and the monuments were cleaned, photographed and measured. A first hand study of the art and inscriptions was undertaken and in both cases the inadequacy of photographs and casts for definitive conclusions was demonstrated. The second expedition will continue the work from this point. The luxuriant tropical vegetation in which the ruins lie buried will be felled and means taken to prevent the annual reappearance of this destructive agent. The laying bare of this site, the clearing of the various pyramids, courts and temples will doubtless be the main work of the present season, though excavations will also be made and the study of the art and inscriptions continued.

The Lake Laboratory of the Ohio State University has announced its courses for the summer session of 1911, and covers practically the same ground as in previous seasons. The staff includes representatives from a number of Ohio Colleges, including Professors Brookner, of Buchtel; Coghill, of Denison; Fullmer, of Baldwin; Osborn and Landacre, of Ohio State University, and Jennings, of the Carnegie Museum in Pittsburgh. with one position, in ornithology, yet to be filled. The subjects covered are general zoology, aquatic biology, invertebrate zoology, entomology,

ornithology, experimental zoology, embryology, general botany and ecology, each in charge of a specialist in the subject. The opportunities of work in these lines are very favorable, the laboratory being located on Cedar Point with access to Lake Erie, on the one side, and Sandusky Bay, with its marshes and open water, on the other side. It is also quite near the islands and to all points of zoological interest. The session opens on June 19, and further information concerning the work or copies of the announcement may be obtained by application to the director, Professor Herbert Osborn, Ohio State University, Columbus, Ohio.

A BIOLOGICAL club has been organized at the Oregon Agricultural College by the faculty and graduate students to make studies of the biology of the state. Professor H. S. Jackson, of the department of botany, was made chairman for the coming year with George F. Sykes, of the department of zoology, as secretary. The club voted to make one of its first problems a thorough biological survey of Mary's Peak, a work which will occupy at least two years. Through the meetings, field trips and collection of material, it is hoped to add materially to the present knowledge of the biology of the state, while at the same time interest will be stimulated in the study of biology.

THE sixty-third meeting of the American Society of Mechanical Engineers will be held in Pittsburgh, Pa., from May 30 to June 2, inclusive. The society has not met in this city since 1884. The headquarters of the society are in New York City, and Col. E. D. Meier, of St. Louis, is president this year. The society has in the Pittsburgh district alone a membership of about one hundred and sixty. Last year the society held a joint meeting in England with the British Society, the Institution of Mechanical Engineers. An executive committee consisting of E. M. Herr, chairman; George Mesta, J. M. Tate, Jr., Chester B. Albree, D. F. Crawford, Morris Knowles and Elmer K. Hiles, secretary, will have charge of the Pittsburgh meetings. There will be professional sessions when papers will be read and discussed. There will also be inspection trips through the leading local industrial establishments.

THE progress of the graduate electrical engineering work at the Massachusetts Institute of Technology is indicated by the number of students who are candidates for higher degrees, which number is now greater than last year. The number of students in the undergraduate course in electrical engineering is also steadily increasing so that additional teaching staff is being added to the corps of laboratory instruction. Various lines of research are being carried on in the department mostly under the direction of Professor Pender and Professor Wickenden. Some of these relate to the effects of heat treatment on the magnetic qualities of silicon iron, certain transient phenomena that may occur in long electric circuits, the effect of high frequencies on the permeability of iron, the effective resistance and reactance of steel rails when conveying alternating currents, the selective action of spark gap lightning arresters with respect to frequency, the reflection of light from walls and ceilings, the disruptive strength of rubber insulated coatings, on wires, etc. Certain of these are continuations of work started last year, and researches in each will be carried on as may be convenient and needful to get knowledge of the phenomena under investigation. The results of the thesis research of Dr. Harold Osborne on whom the degree of doctor of engineering (the first conferred by the institute) was conferred last June, were embodied in a paper presented before the American Institute of Electrical Engineers at its October meeting. The subject of illumination and photometry has been added to the subjects taught in the electrical engineering department. This is treated from the standpoint of what is generally called illuminating engineering and is made an optional study.

IT is reported that the Italian government will establish a Vulcanological Institute, for which the chief governments will be invited to contribute £60,000. Mr. Immanuel Friedlaender, who resides in Naples and is the author of a work on the volcanoes of Japan, has promised, it is said, £4,000 towards this fund.

The second Shaler Memorial Research, supported by the Shaler Memorial Fund of Harvard University, will consist of a study of shoreline changes along the Atlantic coast by Professor D. W. Johnson and two or three assistants. Special attention will be given to changes in the form of beaches within recent geological time, and to supposed evidences of recent coastal subsidence. Since the problem of coastal subsidence is affected by the relative heights of high tides on the outer and inner sides of barrier beaches, lines of levels will be run between the ocean and lagoons, upon which tidal observations will be based. The most important localities from the Bay of Fundy to southern Florida will be examined during the spring and early summer. During the latter part of the summer Professor Johnson will visit localities on the coasts of England, Holland and Sweden, for the purpose of making comparisons with similar localities on the Atlantic coast of North America.

UNIVERSITY AND EDUCATIONAL NEWS

M. Auguste Loutrefil has bequeathed $700,000 to the Paris Academy of Sciences, $500,000 to the University of Paris and $20,000 to the Pasteur Institute.

The University of Michigan has received a gift of $10,000 from William J. Cook, now of New York, and formerly of Hillsdale, Mich., to be used toward the erection of a residential hall for women.

By the will of Miss Susan G. Lansing, of Albany, N. Y., Rutgers College receives the sum of $5,000, together with one third of the residuary estate, which, it is estimated, will bring about $10,000 additional.

The residue under Sir Francis Galton's will is bequeathed to the University of London for the encouragement of the study of eugenics.

The technique of printing and publishing is a new course of study at the University of Wisconsin in connection with the course in journalism. It is designed for students of agriculture, engineering and commerce, who are preparing to enter technical and trade journalism. A class in technical and trade journalism has been organized to give further training in this field.

The University of Illinois special train to rural schools started out for a two weeks' trip over the Illinois Traction system on February 27. The special consists of two cars fitted up with illustrative material for the use of the speakers who accompany the train. About one thousand children are visiting this special every day. The county superintendent of schools of each county that the special visits accompanies the party and acts as guide and director.

Professor V. H. Blackman, of Leeds, has been appointed to the professorship of plant physiology and pathology at the Imperial College of Science and Technology, London.

DISCUSSION AND CORRESPONDENCE

THE USE OF NUMERALS FOR SPECIFIC NAMES IN SYSTEMATIC ZOOLOGY

In a recent number of Science, Dr. Needham has suggested the use of a numerical system of naming species, in addition to the present binomial system devised by Linnæus.

To this suggestion there are several objections, which to the practical worker in taxonomy seem wholly insuperable. In the first place, the name of an animal is not the main element concerned. The specific name covers our conception of the species, a conception likely to be greatly modified by thorough study. The generic name indicates our conception of where it belongs. This conception, of necessity, changes with the progress of knowledge. The changes in name mark such progress. To the taxonomist, certain changes of name are as real and as important as any other forward step in science. It is of course unfortunate that some species have had many different names. So have many genera also. This is due primarily to the inherent difficulties of the subject, as few branches of knowledge are more intricate than the study of the genetic derivation of forms, and their exact geographical distribution. These two branches of science, taxonomy and zoogeography, must

depend for their existence on exactness in nomenclature. Besides this, it often happens that a publication in one nation may be unknown in another, that different writers reach the same results almost simultaneously and independently, and still worse, that some writers are careless or ignorant of the literature, or have felt free to improve on the work of their predecessors by changing, not their conceptions, but the names they have given.

This condition in which anybody called any animal or plant what he pleased went on for more than eighty years after the publication of the "Systema Naturæ." It was evident that all exactness in nomenclature was being lost and that the only way out was through the law of priority and through considering systematic zoology as a democracy in which there was no respecting of persons. Since the first attempt at the recognition of the law of priority in nomenclature, we have come by degrees to relative stability. So far as the first name given to species or a genus was concerned, this name, unless already in use, is right. All the others are wrong. To those who regard rules, the number of names doubtful from the standpoint of nomenclature is now but a very small proportion of the total number. Those zoologically doubtful are naturally far more numerous.

The many zoological problems involved must be settled by observation of the facts in nature, not by rule. There is scarcely a species of which we finally and completely know the actual boundaries. The value and limitation of generic groups changes with every increase of knowledge. Forms once placed side by side are shown to belong far apart. Those far apart are often brought together. In this regard, there can be no stability until the facts are all in. A nomenclature absolutely stable would represent intellectual stagnation.

But to the systematic worker in any field, the actual changes bring no great inconvenience. Names are nothing without ideas. His difficulties do not lie in the remembering of names, but in getting the facts to which names are the handles. The postman is not worried over the fact that each town has a name, and that it belongs to some county, and that there are many counties in many states. If he has troubles, it is not because there are so many names, but because there are so many towns and so many people to be named. So with the taxonomist in any field.

To the worker in other lines in biology, who asks of taxonomy nothing save the name of the animal he is working on, all suspense is aggravating. He wants the scientific name once for all, and he doesn't want it changed. We are sorry that we can not accommodate him, but a name as such is not the main question with the taxonomist. We may let the anatomist keep for his own purposes such names as *Amphioxus,* although the taxonomist can not use it, because the group had a name before *Amphioxus* was invented. The anatomist may in time get used to *Branchiostoma* just as he has become reconciled to *Necturus,* in place of the much later *Menobranchus* once sacred to his purposes.

The fact that a name seems to be in common use just now is no argument for its permanence. The next generation realizing more and more the value of law and order, will discard the name that should not legitimately be used. It is just as necessary in taxonomy and in zoogeography to have a clear-cut nomenclature—above all whim or personal preference—as it is in anatomy to have clean knives, or in histology to have trustworthy staining fluids.

As to the substitution of numbers for specific names or their use in place of such names, we have first the minor objection, of inaccuracy. There will be a dozen errors in a column of figures to one in a column of names, because with the numerals the memory has nothing to hold to. If you live at No. 163 West 135th St. half your letters will be misdirected. This can be easily tested. The dead-letter office is sending back to me letters I directed to 916 East 19th St., and to 919 East 14th St., which should have gone to 914 East 19th, and I have now to write these figures twice to be sure that they are right. No. 256 Knickerbocker Avenue does not have this trouble. Besides, misprints in names correct

themselves. Slips in numbers can not do so.

But waiving all this, the plan seems utopian. Let us look at its application to the group of fishes. There are about 12,500 known species of fishes, arranged in about 2,500 genera. Over 4,000 genera have been named and upwards of 30,000 to 40,000 species. Of these names, perhaps 10,000 are known to be synonyms, the result of some one's misfortune or carelessness. The majority of the supposed species have not been tested. The seas are large, there are many rivers, and but few men who study these animals thoroughly. In our system of numbers shall we count real species or merely count names? Manifestly it is only the names which we can use, for we do not know half the species well enough to assign them a final place. Again, shall we number all species of fishes from 1 to 40,000—or shall we number them by groups or by genera? In any case, a single man or bureau must do all the numbering for all the world, else we should have a crossing of numbers. I might use 38,927 for my cat-fish, while my Russian friend might claim it for a sturgeon. If we number by genera, my *Ameiurus* 36 may not be the same as my friend's *Ameiurus* 36 issued at about the same time. Or one or the other might make an error, or misprint, duplicating what is already numbered.

We must then have in each group a central numbering bureau, a bureau which shall have the means to go back and number all the forgotten species already in literature. We would have to do this before the work could begin. Our American channel cat, *Ictalurus punctatus*, has received some 27 specific names after it was called *Silurus punctatus*. To do it justice, we must refer to it as *Ictalurus*, 5, 27, 36, 38, etc., thus including the whole list of synonyms, any one of which some one some time may show to be valid. But the channel cat was not originally called *Ictalurus*. This raises the question as to whether you would list it as *Silurus*, which it is not, or as *Ictalurus*, which it is, or as *Ameiurus*, *Elliops*, *Synechoglanis*, *Pimelodus* or other generic names under which synonyms its species have been recorded.

Manifestly they must be listed under the original generic name, for no one yet knows the final boundaries of the modern genus. The modern genus consists of a group of species clustering round its original type. The boundaries between *Ictalurus*, the channel cat, and *Ameiurus*, the ordinary cat fish, are still uncertain. There are species intermediate, with the head of *Ameiurus* and the tail of *Ictalurus*, and it may be that the two must coalesce. So the same channel cat may be *Silurus* 25, *Ameiurus* 29, *Pimelodus* 75, *Synechoglanis* 1. Under the law of priority, it can have but one right name. This is *punctatus*, the oldest specific name attached to its right genus, which, as we now understand it, is *Ictalurus*.

But let us start the numbering and see where we come out. Shall we begin with the lowest fish or with the fish first made known? Our system of nomenclature begins on January 1, 1758. The first fish named is the common lamprey, *Petromyzon marinus*. *Petromyzon* offers no difficulty, except that according to Linnæus, *Petromyzon* is not a fish, but an amphibian. His *Amphibia nantes*, or swimming amphibians, in his mind are not real fishes.

Passing on to the first species actually called a fish by Linnæus, *Murœna helena*, the European moray, we have then *Murœna* 1. But this Linnæus *helena* obviously is not a species. It is a compound of what is now called *Murœna helena*, identifiable from its use at the suppers in honor of Helen in Rome, to which Linnæus refers, and of two other species, one of the old world, one American. *Murœna* 1, therefore includes *Murœna* 50 (—*Gymnothorax moringa*) and *Murœna* 90 (*polyzonia*). But we will use the name *helena* for the Roman moray *Murœna*. *Murœna* 2 (*ophis*) is—no one can tell what—a species of *Ophichthus*, and *Murœna* 3 (*serpens*) is the type of the later genus called *Ophisurus* or *Oxystomus*. It has very little in common with the morays. Have we gained much by substituting *Murœna* 1, *Murœna* 2 and *Murœna* 3, for *Murœna helena*, *Ophichthus ophis* and *Ophisurus serpens*?

But perhaps it will be best to begin at the bottom of the series. The lancelet is the lowest fish, if (1) it is a fish, and (2) if the *Tunicates*, and the *Balanoglossi* are not also fishes. If we number the fishes from 1 to 40,000, we shall have to decide beforehand as to the nature of tunicates, lancelets, lampreys, chimæras and sharks as well as that of their various extinct relatives. Apparently the only safe way will be to number the species after another, each in the genus in which it was originally placed. In that case, the genus may go where it will, the species will hold their numbers.

In 1774, Pallas named the lancelet, *Limax lanceolatus*. But it is not a *Limax*. *Limax* is a land-slug. Must we wait till other shell-less snails or *Limax* are numbered, before we can list our first fish. Let us chance it as *Limax* 75 and keep it with the fishes if we can.

In 1834, Costa named this same lancelet *Branchiostoma lubricum*. *Branchiostoma* 1 is therefore equivalent to *Limax* 75. But the species should not be called *lubricum*, but *lanceolatum*. This Yarrell recognized in 1836, calling it *Amphioxus lanceolatus*, bringing up the old specific name. But his generic name, new and useless, has been the source of much subsequent trouble. In any case the species is not *Amphioxus* 1, because it does not start with *Amphioxus*. It was known sixty years before the time of Yarrell.

Our next fish is *Branchiostoma caribæum* of Sundevall in 1853. This is a doubtful species, most likely the same as *B. lanceolatum*, but it may stand as *Branchiostoma* 2. *Branchiostoma Californiense* Gill 1893 may be *Branchiostoma* 3, and the remaining lancelets are scattered over the world, some recorded as *Amphioxus*, most as *Branchiostoma*.

It is not necessary to follow this further. The same conditions prevail throughout zoology. The fact is that our present Linnæan system of naming species and groups in zoology or botany is still the best which has been devised or suggested. It has the right of way through one hundred and fifty years of usage. All present taxonomy is based upon it. Its embarrassments are due chiefly to the difficulties inherent in the subject, and to the limitations of human nature.

The changes in names of the last thirty years have been, on the whole, in the direction of final stability. The zoologists of the world have devised machinery which will steadily make for permanence, and the necessary period of transition is one from lawlessness to law, from confusion to science. In so far as we have confusion this has arisen through neglect or ignorance of law. It can not be remedied by further neglect. A writer dealing with scientific names must either call an animal or plant whatever he pleases, or else he must conform to regulations inherent in the nature of his work. Arbitrary rules will soon be disregarded. The necessary regulations are those which future workers will approve, and we, who are still working in the infancy of taxonomy, must lay foundations on which the future can build.

In view of the great issues which depend on accuracy of method, such minor issues as that we rather say *Amphioxus* than *Branchiostoma*, or that it suits us better to call the common eel *Anguilla vulgaris* rather than *Anguilla anguilla*, or that our collection is labeled according to the method of Cuvier, sink into insignificance. You can say *Amphioxus* if you like—or *Bdellostoma*. We shall know what you mean, but we shall not try to force these names back into nomenclature, replacing older and legitimate names already becoming better known to the actual worker in taxonomy than these names of temporary convenience ever were or ever will be.

DAVID STARR JORDAN
STANFORD UNIVERSITY

THE USE OF SYMBOLS IN ZOOLOGICAL NOMENCLATURE

AT first thought, Dr. Needham's suggestion[1] that in substance we designate what are practically subgenera, species and so on, by symbols does give more or less of a shock. Never-

[1] SCIENCE, N. S., XXXII., pp. 295–300, September 2, 1910; see also *ib.*, pp. 428–429, September 30, 1910, and XXXIII., pp. 25–29, January 6, 1911.

theless, a little thought certainly shows that some such system as this may be a necessity in the near future and, if for no other reason, should receive earnest attention and discussion. The system proposed by Dr. Needham has obvious advantages: By grouping closely related genera (becoming subgenera) under the old name of the genus when used in its widest sense, two of the fundamental reasons for the existence of nomenclature are reached, namely, stability and ease in identification and in grasping the relations of the various units at a glance. But, to my mind, the system has nothing at all to do with stability unless this fundamental change is instituted. All will grant, I think, that stability is fundamental, as is also ease or at least possibility of identification. I believe, too, that all will concede that neither is possible without what may be called "rigidly" defined genera (=groups), genera which all are willing to rank as such and which all will be able to recognize (perhaps they would be equal to present-day subfamilies at least).

These genera or groups being firmly established by universal acceptance and concise description, then the application of the symbols would doubtless save an immense amount of space. Otherwise, I am certainly at a loss to find any other advantages which they may have. Synonymy nor anything else is simplified by saying that $5 = 4$ instead of *leucopsallis* = *viridis*. The only thing that matters is whether the statement is true or not. You may call 5 anything that you wish without changing what it represents. And is it not true that most of our troubles cluster about the fact that we have been unable to find out what authors have meant to represent?

The objections to involved nomenclature entered by the zoologist and biologist are entitled to much consideration, but we should not lose sight of the fact that the present systematic unit—the species—was founded by themselves and seemingly we still find an endless number of them. If it is true that they exist it is our duty to keep on recording them. Whether we call them by symbols or names isn't to the point at all. The gist of the matter is, shall the conception of the systematic unit be changed from "natural" species to conceived genera? Will any biologist deny that species exist. Why, therefore, should they wish to escape from them? It is true it is impossible to know all of them nor even their names! But who wants to do this. The fact that they exist is true, or else our conception, or rather perception, of a species is all wrong. Now, if it is true that they exist, I believe that it is necessary that they be represented by names or else symbols. Thus, whether names or symbols are used, either would have to be used an equal number of times, but the symbols would be shorter, that is all. It is not the jungle of names that masters us, is it? Rather, is it not the jungle of things? To simplify, natural laws, not symbols, are needed.

Therefore, it seems to me that the fundamental plan suggested by Dr. Needham, that of falling back upon the old genera and their names, is the only way out of the confusion, present and past. As for the symbols, they are preferable only in so far as they have a tendency to simplify, not our knowledge, which they are certainly unable to do here, but our working methods, time and space.

A. Arsène Girault

Urbana, Ill.,
January 9, 1911

ON FACTORS CONTRIBUTING TO A LOW SCIENTIFIC PRODUCTIVITY IN AMERICA

A few months ago I offered some criticisms on a paper by Professor Gunn which appeared in Science for October 28, 1910, under the caption, "American Educational Defects." My criticisms were directed chiefly to the method adopted by Professor Gunn, and he has very properly retorted[1] that I should not make too much of the matter of method unless I am prepared to dissent from the practical outcome of his study.

Now so far as this outcome was to the effect that the level of scientific and scholarly productivity in this country is unsatisfactory by comparison with that in certain European

[1] Science, January 20, 1911, N. S., XXXIII., 107.

countries, I am not prepared to dissent. I do indeed believe that Professor Gunn's picture is overdrawn, when he describes our achievements in pure science as "insignificant," for it is easy to point to achievements of very high grade, even in such branches as mathematical physics and philosophy, while of recent years there has appeared a considerable volume of quite respectable work. Still, I should admit that the work of very high quality has been too small in amount, and that the volume of recent work suffers somewhat in an appraisal of its quality. I should indeed be inclined to make a further reservation on this last point, as far as my own acquaintance with scientific literature goes; for in my own field of experimental psychology, which has hitherto been chiefly cultivated in Germany and in America, I am unable to detect any pronounced superiority of the German work. The Germans do, certainly, manage to give their contributions a more important sound; their articles are more extended, and run out almost indefinitely into discussion and theoretical considerations; but much of this is of little real value, and many an American paper of modest length contains as much of real contribution to knowledge as does its German analogue of a hundred or two hundred pages. However, let us freely admit that, when we consider the number of men here who might be expected, from their training and their positions, to be scientific producers, we find the total productivity surprisingly small. There is much to indicate that this is the fact: so numerous are the cases of young men who have produced a creditable doctor's dissertation and obtained a college position in their specialty, but from whom nothing further is heard in the way of original contribution; and so numerous also are the cases of men of proved ability, who, after a few years of activity and after winning a professorship of dignity, allow their output to cease. Good minds and good opportunities appear to be going to waste, and the problem of the causes of this condition is one of the highest importance to those who are interested in the advancement of science. It is a problem which deserves treatment by the most painstaking methods of science; unfortunately, I can make no great claims for my own method, for I have by no means conducted researches on the large scale demanded by the complexity of the problem. I have, however, for a considerable number of years been keenly interested in this particular problem, and am prepared to adduce a certain number of facts, which, as facts, will scarcely be called in question, and which I shall try to show are probably pertinent.

I will first adduce my list of facts, in summary form.

1. The economic rewards for scientific production, and punishments for lack of it, have been smaller here than elsewhere.

2. Similarly with other social rewards and punishments.

3. The rapid expansion of our educational system has created a demand which has absorbed the whole supply of even reasonably qualified men.

4. This educational expansion has been but a feature of the general national expansion, and the general demand for men of ability has operated still further to reduce the keenness of academic competition, and so to lower the standard of academic success.

5. This rapid expansion, in the presence of our decentralized form of governmental control and generally fluid condition, has made the business of the educational and scientific promoter one of great importance, has operated to give the greatest economic and social prizes to the promoter, and has caused scientific men to spend their time running errands in the interest of science rather than prosecuting their individual research.

6. The educational interest, as distinguished from the strictly scientific, has been strong among us, and has led to a considerable deflection of effort from the work of science.[2]

[2] There is another probable fact, which I do not include in the list because I am not sure of it, and because it could be determined by suitable inquiry, in advance of which it is best not to guess at the fact. The probability is that our young men do not begin to specialize so early as their scientific brethren in Europe, and if this is

Lest I should be accused of altogether neglecting principles in my zeal for facts, I will also mention a few general principles which can properly be employed in reasoning from the above facts:

1. The law of supply and demand.

2. The law of the value, as incentives, of rewards and punishments.

3. The law of divided energy, according to which a man can not do so much in a given line if his time and energy are largely devoted to something else.

The great fact of rapid expansion is perhaps the most important of all. Since the most obvious feature of this expansion has been that of the economic development of the country and of the growth of industries, the fact is usually hit off as commercial expansion, and the effort made to deduce all our peculiarities and deficiencies from our commercialism. But the real fact is expansion, a fact, it is probably of great importance. Our own delay in getting the young man fairly launched on his scientific career is partly due to our superstition that the traditional four years of college marks a minimum of time to be devoted to "general culture," after which, only, should specialization begin. Meanwhile, through the raising of the standards for admission to college, the period of specialization has been deferred to about the age of 22. But besides this, it often happens that a man just leaving college and bent on a scholarly career is led to believe that the best step for him next to take is to teach in a secondary school; and thus the age at which he enters on really advanced study is likely to be delayed to 25. From observation of men studying for their doctor's degree, I am convinced that the man who goes straight on from college to the university is usually the one who comes off best in his graduate study. The years immediately following the age of 20 are of great value for the ready assimilation of knowledge, and, moreover, the most original period of a man's life is likely to follow close upon these years; and unless he has good command of his specialty by the age of 25 or 27, he is rather unlikely ever to have many original ideas on the subject. I am convinced that specialization, for any young man whose bent towards a scholarly pursuit is sufficiently marked to warrant urging him to undertake it, should not be delayed much beyond the age of 20. not commercialism—expansion in all directions. A necessary result of this expansion, and a result abundantly in evidence, is that the demand for labor of all kinds, and not least for the labor of intellectually able men, has been great in relation to the supply. The economic reward for intellectual ability has, of course, been much greater in many other lines of work than in the academic, and this has certainly further limited the supply available for scientific pursuits. For example, it has been, and is, difficult to man the laboratory departments of our medical schools, for the reason that the rewards awaiting the successful physician, in practise, have been far in excess of anything he could hope for in research. The financial reward for scientific work is everywhere less than the reward for equal accomplishment in other lines; but here this difference is accentuated. In spite of this fact, scholarly pursuits continue to attract a very considerable number of really able men. The men are attracted in part by the freedom of the academic life, in part by the undoubted prestige attaching to good academic positions, and in largest measure, no doubt, by the work itself. Improvement of the general economic status of university and college teachers is of course greatly to be desired in the interests of broadening the labor market for this highly important sort of work; but that is by no means the key to the whole situation, for we are confronted with an able body of men, men who have proved, in many cases, their ability in original work, but who nevertheless leave much to be desired in the way of productivity.

The expansion of our educational system has, if anything, outstripped our commercial expansion. Universities have multiplied and grown enormously, teaching forces have been greatly augmented, and the demand for high-class men to fill academic positions has been ever on the increase. The demand has been large in proportion to the supply, so that every moderately equipped candidate has been assured of a post of some dignity. Promotion has been rapid, as far as it goes. In other words, the labor market for all grades of academic work has been relatively narrow, and

there has been an absence of keen competition either for the lower or for the higher positions. This is a necessary result of expansion, and, at any rate, it is a fact. The conditions, as regards competition, are very different in some European countries. A young man there must often serve a long apprenticeship in a very poorly paid position, and can only rise out of this difficult situation by overcoming keen competition. Our rather tame discussions of the work of our colleagues lack the keen note of economic competition which is often heard in European controversy. Here, we feel, there is room enough for all, and on an approximate equality. Here it makes comparatively little difference to a man, economically, whether his scientific work is mediocre or of eminent success. For while the ratio of demand to supply assures him of at least a moderately good position, there is nothing in the way of a very fine position to spur him forward. While mediocre men are better off here than in several other countries, very good men, in purely academic positions, are by no means so well off as elsewhere. In Great Britain, at least, there is a considerable number of professorships the financial value of which, when allowance is made for the different purchasing power of money, is fully the equivalent of eight to twelve thousand dollars. The financial value of these posts is well known throughout the kingdom, and, as they are permanent establishments, and are filled, when they fall vacant, in the open market, they act as a very effective stimulus to productivity. They act as a stimulus to a class of men whom it is most of all important to stimulate, and who, in our country, are subject to no such incentive—namely, to the men of greatest ability, who have already proved their power and have already achieved positions as good as any we have here to offer. Not only a high money value, but also great prestige, attaches to some of these chairs, because of the eminent men who have occupied them in the past. We have practically nothing to correspond to them; and this is, I believe, one of the great deficiencies of our system. Nowhere, it would seem, is the punishment for idleness so light as in our academic life; and nowhere is the reward of productive industry so meager. I am far from contending that the mere financial reward is the sole stimulus to scientific production; but these prizes not only bring great financial relief; they are also the seal of success. I might paraphrase what I said a few sentences back by asserting that nowhere is there such a lack, as in our American academic life, of the tangible symbols of success and failure in scholarly work.

To punish mediocrity is scarcely within our power during a period of rapid expansion; but to reward proved merit is in our power. Why should not a university, numbering among its professors some one of the acknowledged leaders in American productive scholarship, simply double or triple his salary, at the same time doing all it can to strengthen his department, and thus secure to itself preeminence in that particular subject among all our universities; insuring, further, a continued preeminence by permanently establishing this distinguished chair and this thoroughly equipped department? It should be possible in this way for a university to attract a large share of the best graduate students in this department, and thus add further to the influence of the chief and to the attractiveness of his position. The combined prestige, influence and financial desirability of such a position would make it a prize for the competition of the ablest of the younger men. There is no reason why such prizes should not act as effective spurs here as elsewhere. Our effort has been devoted more to raising the general level of compensation and attractiveness of all professorial positions than to the recognition of eminent scholarly and scientific success. Certainly there is abundant need for raising the general level of salaries to keep pace with the changing ratio between money and other commodities. But the reward of eminent merit is a thing apart.

Another consequence of rapid expansion, under the decentralized and rather unorganized conditions of our national activity, in which such an interest as the educational must look out for itself, has been the evolution of

the organizer, agent and promoter. The most striking instance is the university president or chancellor. His function has been distinctly that of the promoter; and so important has this function appeared in a period of expansion that the largest rewards, both pecuniary and in the way of social standing and influence, have gone to the presidency, and some of the ablest and most efficient from the professorial ranks have been drafted into administration. Since the duties of the president have been too exacting to allow a continuance of scholarly work, the result has no doubt been a considerable shrinkage in the volume of possible production. Further, ambitious young professors, observing which way the path of distinction led, have often set themselves to prove their ability in administration rather than in scientific production. Administrative opportunity has abounded throughout the educational system, and many who entered the system from love of science or literature have found their attention largely absorbed by matters of management and promotion. Much of this bustling administrative activity has been a necessary result of expansion, but much of it has been due to mere contagion and mutual emulation. The center of competitive activity has been shifted from scholarship to administration. Now all administrative work, however necessary in the circumstances and however ably performed, is but a means to the ends of scholarship and of education; and it seems a pity that so much of the best brains should go to the means and so little be applied directly to the ends in view. The head of a department, instead of entering his laboratory with the thought of his experiment uppermost in his mind, is first of all oppressed by the condition of his desk. When that is cleared up, he hopes to go ahead with his investigation; but the desk occupies him for so large a part of the day that the experiment is deferred till to-morrow. There is a tremendous dissipation of energy among university professors. We are always busy, but seldom get down to business. We are always busy trying to insure that the work of science be done, and leave little time to do the work ourselves. We are so much occupied in contributing to the advancement of science that we are unable to make contributions to science.

The attention of our scholars has been deflected by educational as well as administrative interests. I am inclined to regard this, too, as a consequence of expansion. For our higher institutions of learning have expanded in faster ratio than the general population, and this means that we are undertaking to educate many who are not specially suited to a higher education. Since the net has been made finer, we are catching many small fish, and the educational problem is largely concerned with these small fish. Whatever be the explanation, there is no doubt of the fact that our university professors are more occupied in the effort to impart instruction and insure that the student derives some benefit from it than is the case in foreign universities. I have heard it said that whenever a group of European university men get together, they talk science, whereas we talk education. We are greatly concerned about the student, and largely about the poor student. This may be best in the circumstances, and I have no desire to attempt a rough and ready solution of so complicated a problem; but simply point out the undoubted fact that here is a factor in our comparative lack of scholarly production. With both the administrative and the educational interests so strong among us, we are prone to hover in the outskirts of scholarship, instead of plunging into the heart of it.

There is another aspect to the whole matter, for the universities are not the sole repositories and organizers of scholarship. Guilds of scholars have to be considered as a means of exciting to productivity. We have, indeed, few productive scholars outside of the universities, though this is at least partly due to the prestige which university professorships have among us, for it would be easy to name a score of scholars and scientific men who, though of independent means, have sought university connections, in order to have a definite standing in the scholarly world. College loyalty has been a strong force among us, and

the attachments of a professor have been mostly to his university rather than to the fellowship of his particular science. Of recent years, with the organization of national scientific societies, some change has occurred in this respect. It is to guilds of scholars, whether formally organized or not, that we must look for setting the standard of scholarly production. The fellowship of scholars can only be a matter of gradual development, and their standards also must grow and can not be suddenly and artificially raised; but there is plenty of evidence that the standards of our scholarly guilds have been rapidly improving, and they will probably continue to improve. Such guilds possess rewards and punishments of their own, for the standing of a man among his fellows is one of the strongest incentives to action. The standards of the guilds must eventually be the standards of the universities; and thus we hold in our own hands, quite apart from the momentary attitude of university authorities, a force capable of raising the level of our own work and that of our successors.

R. S. WOODWORTH

COLUMBIA UNIVERSITY

BIOLOGICAL TEACHING IN SECONDARY SCHOOLS

A MEETING of men interested in the advancement of biological teaching in secondary schools was held at the Harvard Union, Cambridge, on Saturday, February 4. Those present were Professor G. H. Parker, Harvard University; Principal Irving O. Palmer, Newton Technical High School; Dr. H. R. Linville, Jamaica (N. Y.) High School; R. H. Howe, Jr., Middlesex School; Samuel F. Tower, Boston English High School; S. Warren Sturgis, Groton School; Head Master Frank E. Lane and W. L. W. Field, Milton Academy. The relation of school biology to civics, the sequence of laboratory experiments, outdoor work with classes and college requirements were the topics informally discussed. The undersigned was authorized to communicate with other teachers with a view to establishing a series of conferences, perhaps to be held alternately in Boston and New York. Correspondence is accordingly invited from interested readers of this notice.

W. L. W. FIELD

MILTON ACADEMY,
MILTON, MASS.,
February 6, 1911

SCIENTIFIC BOOKS

Questioned Documents. A Study of Questioned Documents with an Outline of Methods by which the Facts may be Discovered and Shown. By ALBERT S. OSBORN. With an Introduction by Professor JOHN H. WIGMORE. Two hundred illustrations. Rochester, N. Y., The Lawyers' Cooperative Publishing Co. 1910. Pp. xxiv+501.

"Questioned Documents" is an admirably clear presentation of the application by experts of modern scientific methods to the study of handwriting. It gives a detailed exposition of the use in the identification of handwriting of enlarged photographs taken in various lights, of the document microscope and of the color microscope designed for recording the tints and shades of ink. The instruments and appliances used in getting accurate measurements of such details of writing as the width of the line-stroke and the slant of various parts are also described. Particularly interesting is the suggestion of the new application of stereoscopic photography in such a way as to determine in disputed handwriting the sequence of crossed lines, the time-relation of writing to folds in paper and the presence of erasures and changes in paper-fiber.

The purpose of the book is practical—a very successful attempt to present the science of handwriting in relation to law, an attempt which constitutes a new and profitable departure in legal literature. The author would arouse the interest of the trial lawyer in, and his intelligent comprehension of, the problems involved in questioned documents, so that he may be better qualified to deal with situations involving such matters. Those interested in the pure science of handwriting will, none the less, find much to learn from the author relative to its accurate measurement and analysis. The reviewer is acquainted with no other

treatment of the subject from the practical standpoint as thoroughgoing and suggestive.

Since the psychology of individual variation in handwriting characteristics is still an unwritten chapter of the science, it is not surprising that the analysis of handwriting habits in the volume under consideration should be largely in terms of the writing system learned by the penman and of the writing instruments and material utilized by him. Such an analysis is accompanied by an historical account of the rise of various systems of handwriting and by a description of their characteristics. The dependence of many peculiarities of writing, such, for example, as shading, upon pen position, should be noted by the investigator of the subject. The author insists upon the use of a sufficient amount of proved handwriting as a standard for comparison in the case of a disputed document and records instances of normal variation in handwriting in such a way as to show forgery by a tracing-process in the case of unnatural uniformity. The interesting observation is made that individual writing habits are found to be revealed more clearly in minor details than in striking features, such as large capital forms. Possibly the author might, with profit, have treated at greater length variations in handwriting due to age, disease and emotional disturbance.

The author insists that the testimony of the handwriting expert should, if acceptable, be the expression not of an opinion founded upon more or less vague intuitions, but of a scientific conclusion from facts, a conclusion based upon reasons which are intelligible to the non-expert and presentable in court. The author is sceptical of testimony that concerns itself with the general appearance of handwriting rather than with accurate analysis and measurement. He is, naturally, amused by the pretensions of the graphologists who would read from handwriting the physical characteristics of the penman and catalogue therefrom his vices and virtues.

The application by the author of the methods used in identification of handwriting to the study of questioned typewriting shows a new field of inquiry, one that appears well worth working by the expert.

JUNE E. DOWNEY
UNIVERSITY OF WYOMING

Tables for the Determination of Common Rocks. By OLIVER BOWLES, M.A., Instructor in Geology and Mineralogy, University of Minnesota. 16mo. Pp. vii + 64. New York, D. Van Nostrand & Co. 1910. $0.50.

This text is designed to meet the need of suitable tables for the determination of rocks and rock-forming minerals by microscopic methods and constitutes a convenient and useful pocket guide for field and laboratory purposes.

The usual classification of rocks is given but no attempt is made to group them in the tables accordingly. The grouping, based upon texture, is I., Glassy; II., Ashy or Cellular; III., Crystalline, even grained; IV., Porphyritic; V., Dense and Finegrained; VI., Banded; VII., Fragmental. The various types are arranged in the proper group and described briefly. In the case of crystalline rocks, mineral composition is made a basis for further subdivision and one chapter is given to tables for the determination of the more common rock-forming minerals, the classification being based upon color, hardness and cleavage.

The last chapter contains a short discussion of building stones. Terms used in the text are amply defined in a glossary at the end of the book.

R. W. CLARK
MINERALOGICAL LABORATORY,
UNIVERSITY OF MICHIGAN

SCIENTIFIC JOURNALS AND ARTICLES

THE contents of the *American Journal of Science* for March are:

"Transmission of Light through Transparent Inactive Crystal Plates, with Special Reference to Observations in Convergent Polarized Light," F. E. Wright.

"Separation and Estimation of Barium Associated with Calcium and Magnesium, by the Action of Acetyl Chloride in Acetone upon the Mixed Chlorides," F. A. Gooch and C. N. Boynton.

"Feldspar Aggregate Occurring in Nelson County, Virginia," W. M. Thornton, Jr.

"History of the Coconut Palm in America," O. F. Cook.

"New Mink from the Shell Heaps of Maine," F. B. Loomis.

THE first number of the new journal, *Phytopathology,* has just appeared. This periodical is the official organ of the American Phytopathological Society. It is to be published bimonthly and to be devoted to both the purely scientific and practical economic features of plant disease investigations. The chief editors are Dr. L. R. Jones, professor of plant pathology, University of Wisconsin; Dr. C. L. Shear, plant pathologist, U. S. Department of Agriculture, and Professor H. H. Whetzel, professor of plant pathology, Cornell University; who are assisted by twelve associate editors, representing different institutions and sections of the country. The initial number contains 37 pages and 6 plates. An excellent portrait of Anton de Bary, hitherto unpublished, appears in the frontispiece. The following articles are included:

"Anton de Bary" (with portrait), Erwin F. Smith.

"The Rusts of White and Red Clover," Frank D. Kern.

"Crown Gall of Plants," Erwin F. Smith.

"Fig Diseases," C. W. Edgerton.

"Floret Sterility of Wheats in the Southwest," Edw. C. Johnson.

"Black-leg or Phoma Wilt of Cabbage," Thos. F. Manns.

"A New Fruit Spot of Apple," W. M. Scott.

Reviews.

SPECIAL ARTICLES

A KINETIC THEORY OF GRAVITATION[1]

EVER since Sir Isaac Newton enunciated the law of universal gravitation, more than two hundred years ago, philosophers have speculated on the nature of the mysterious agency which links every atom of matter in the universe with every other atom. Newton was unable to offer any adequate explanation.

Since Newton's time several theories of gravitation have been proposed, but all, of which I am aware, are open to strong objections and are not considered even promising by physicists.

Study of the nature of gravitation is beset with unusual difficulties; because gravitation is ever with us and about us, it is the one universal phenomenon, and we can not escape from its influence—can not obtain any outside point of view.

Gravitation is often described as a feeble force; and so it is, from one point of view. It is difficult to measure, or even to detect, attraction between two small bodies. But when the bodies are of planetary size the aggregate attraction of their molecules is enormous. It is easy to calculate that the attraction between the earth and the moon, which is just sufficient to retain the latter in its orbit, would, if replaced by a steel cable, require that the cable be about five hundred miles in diameter in order to withstand the strain. Between the earth and sun, the cable would have to be nearly as large in diameter as the earth; and attraction between the components of some double stars is millions of times greater than between the earth and sun (Lodge). So tremendous a phenomenon as gravitation, a phenomenon compared with which all others seem trivial, must have a mighty origin.

That gravitation is a phenomenon of the all-pervading ether is beyond reasonable doubt. This is so generally conceded that it need not be argued. But how does the gravitative influence originate? how is it transmitted and maintained? what is the *mechanism* of gravitation? It is the purpose of this paper to attempt an answer to these questions.

Let us consider what happens to a falling body. We know that it gathers kinetic energy from some source, as evidenced by its acceleration; that this energy may do external work or develop heat; that the amount of energy gathered is measured directly by the distance fallen through (within the limits of uniform gravitation), irrespective of the time or rate of falling. When the distance fallen through is of inter-planetary magnitude, and the attracting body large, the gathered energy

[1] Read before the American Association for the Advancement of Science, December, 1910.

is enormous; sufficient, if converted into heat, to vaporize the most refractory falling body.

We are here confronted with the question, whence comes the energy acquired by a falling body? Certainly it was not inherent in the body before the fall, as evidenced by the fact that during unimpeded fall none of the physical or chemical attributes of the body, aside from the acquired motion, changes in the slightest degree.

We have been taught that before the fall the body was endowed with "potential energy of position," which is converted into kinetic energy during the fall. I think "energy of position" is an unfortunate term because it is so very inadequate. To me, it explains nothing. The case is not like that of a flexed spring, where there is internal molecular strain or displacement.

Let us imagine a pound-weight of iron, for instance, raised from the surface of the earth to a point near the moon, in a line joining the centers of the two bodies, the point so chosen that the opposing attraction of the earth and the moon shall exactly balance each other, leaving orbital motion out of consideration.

On the surface of the earth the pound-weight had some so-called "potential energy of position" because it was capable of falling into a pit; but in its new position near the moon, this potential energy not only has not been augmented, but has disappeared entirely; the pound-weight, left free to move, remains stationary. And yet we must have expended more than twenty million foot-pounds of energy in overcoming the attraction of the earth and lifting the weight to its new position. This amount of energy would be sufficient to impart to the weight a velocity more than ten times greater than that of the swiftest cannon ball; or, if converted into heat, would be many times more than sufficient to raise the iron weight to dazzling incandescence and then vaporize it. Now, in lifting the weight, this large amount of energy has disappeared utterly. We can not believe that the whole or any part of it has been annihilated; it must, in some form, be resident somewhere. I think no one will contend that this energy is resident, in any form, in the cold, motionless pound-weight. I believe it was absorbed by, and is now resident in, the ether through which the weight was raised. Conversely, if this be true, a falling body must acquire its energy from the ether through which it falls. This is a fundamental idea to which I invite attention. Faraday glimpsed it long ago, and others have appreciated it more clearly since his time. But, so far as I am aware, no one has realized its significance.

This view of gravitation implies that the ether is endowed with very great intrinsic energy in some form. Many scientists now hold that the ether is so endowed, and that the amount of this intrinsic energy is enormous. Sir Oliver Lodge[2] appears to regard this energy as potential in form, and estimates the intrinsic energy of a single cubic millimeter of the ether to be almost inconceivably vast. He says, "All potential energy exists in the ether." Sir J. J. Thomson says, "All kinetic energy is kinetic energy of the ether."

I conceive the ethereal energy involved in gravitation to be kinetic rather than potential, the latter involving strain or stress. Newton, and later Maxwell, assumed that bodies produce a stress in the ether about them, of such nature as to account for gravitation; but they were unable to imagine any physical cause for the stress.

All the past theories of gravitation of which I am aware, except the corpuscular theory of La Sage, appear to regard gravitating matter as the seat of the gravitative influence, the surrounding ether, by induced stress or otherwise, acting simply as the medium of transmission. I can not see that any of these theories accounts for the energy acquired by a falling body.

My own view of gravitation differs from these radically. I believe that kinetic energy of the ether is the fundamental cause of gravitation; and that a gravitating body plays a secondary rôle only, in disturbing the normally uniform distribution of the ether's energy, in a manner I shall endeavor to explain later.

[2] "The Ether of Space."

Let us assume, then, that the ether is endowed with very great kinetic energy normally uniform in distribution.

Kinetic energy implies motion of something possessed of inertia. Now, inertia is a fundamental attribute of the ether. Sir J. J. Thomson holds that all inertia is inertia of the ether. The ether is highly elastic also, which, with its inertia, enables it to possess kinetic energy in wave form, as exemplified in radiation. By the term wave, I mean progressive motion locally periodic; doubtless the ether as a whole is stationary. Hence we may consider the kinetic energy of the ether as consisting in ether waves of some kind.

These waves, vast in aggregate energy, eternal in persistence, without finite source or destination, are imagined as being propagated in straight lines in every conceivable direction. This isotropic distribution of kinetic energy, essential to my theory of gravitation, was, for me, a difficult conception until I reflected that isotropic radiant energy is approximately realized in the interior of any furnace with uniformly heated walls.

Any kind of waves capable of exerting motive action on the atoms or molecules of matter will fulfil the requirements; but I shall first consider the transverse, electromagnetic waves of radiation because these are the kind of ether waves we are familiar with.

Of course intrinsic ether waves, if of the radiation kind, can not be of any frequency at present known to us as radiation, because then all bodies would become heated. But we can easily imagine them of such extremely low frequency that the molecules or atoms of matter can not respond to them—can not vibrate in unison with them—molecular resonance can not be established; hence no conversion of the ether energy *directly* into heat in the ordinary way can take place.

We are familiar with the dissipation or degeneration of the higher forms of energy into heat, and the continual degradation of heat to lower degree; that is to say, less violent molecular vibration and more general distribution. As is well known, it is only through this degradation or running down of natural energy that we are enabled to utilize some of it. Lord Kelvin called this function of energy "motivity" (we now call it entropy), and said the motivity of the universe tends to zero.

We know that ordinary radiation waves in the ether persist indefinitely and without change of frequency or direction until they encounter matter, when they are absorbed and converted into heat, only to be radiated again, usually in longer waves, to some colder body. This degradation of wave frequency continues until we can no longer follow it. I beg to suggest that the ultimate destination of this wave energy is that vast reservoir of kinetic energy intrinsic to the ether. We may liken the waves of radiant energy, which we apprehend as light and heat, to wind ripples on the surface of water, which continually degenerate in wave frequency until they are absorbed into and become a part of the mighty swell of the ocean.

Thus we may, perhaps, regard the ether's intrinsic energy as energy in its lowest form —Kelvin's zero of "motivity." But fortunately we may and do get some of this energy back in available form in several ways, as, for instance, when a falling body is arrested and develops heat; some of our wind ripples are then returned to us.

When two gigantic astronomical bodies collide under the influence of gravitation, as sometimes happens, we witness in far distant space the birth of a nebula. The inconceivably vast amount of heat developed by the collision converts both bodies into luminous vapor which expands with incredible rapidity into the nebulous cloud. This heat energy must in course of time degenerate back into the ether whence it came, though billions of years may be required; and during all this time the energy has "motivity." We may picture the stupendous result of the collision as only a local splash in the ether's mighty ocean of energy.

Having postulated that the ether is endowed with very great intrinsic kinetic energy in wave form of some kind; that the waves are

propagated in straight lines in every conceivable direction, *i. e.*, the wave energy is isotropic; and that this energy is distributed uniformly throughout the universe except in so far as the distribution is disturbed by the presence of matter, I shall endeavor to explain my conception of the mechanism of gravitation.

For illustration in terms of the known, let us imagine a closed space having uniformly luminous walls of such character that every point on their surface radiates light in all internal directions. The enclosed space may be of any shape, but for the sake of simplicity let it be spherical or cubical, and large, say as large as a lecture room. The space will be filled with isotropic radiant energy uniformly distributed—any cubic centimeter of space containing as much energy as any other.

Next let us picture a small opaque body suspended anywhere in our luminous space. The body may be of any shape we may imagine an atom or molecule to have; but, again for simplicity, let it be spherical—say a small grain of shot, and let it be located near the center of the space.

The small body will absorb the light which falls upon it and will cast a spherical shadow, the depth or intensity of which will vary inversely with the square of the distance from the center of the body; and the shadow will extend to the confines of the enclosure, however large the latter may be. We can not perceive the shadow, but we know it is there. It is true that the body will soon acquire the temperature of its surroundings, and radiate as much energy as it receives; but for the purpose of this illustration let us consider only the high-frequency light energy.

As is well known, the ether waves of light will exert a slight pressure on the body. But in the case supposed, the pressure will be equal on all sides and no effort toward translation can result.

Now let us introduce a second small body, similar to the first, and some distance from it. This, also, will cast a spherical shadow like the first. The two shadows will intersect, and each body will lie within the shadow of the other. In other words, each body will be partially shielded by the other from the ether waves coming from that direction. Hence the light pressure will be less on that side of each body which faces toward the other than on the side which is turned away, and the bodies will be urged toward each other by the excess of light pressure on the sides turned away. This excess of pressure will vary with the inverse square of the distance between the centers of the bodies so long as the ratio of distance to diameters remains large.

The ether waves concerned in gravitation can not, however, be like the light-waves I have just used for illustration, because light-waves heat bodies on which they fall; and their pressure is almost wholly superficial, it does not reach molecules much below the surface, and hence bears little relation to mass.

But let us substitute for the short and feeble waves of light, powerful waves, still of the radiant kind, but of such great length and slow frequency that, as before explained, they do not excite the molecular vibrations which we appreciate as heat, and hence are not absorbed by matter; they pass freely through all bodies, bathing the interior molecules as effectually as those on the surface.

Under these conditions each molecule or atom or unit of a gravitating body will have its own spherical shadow or field of influence, and the gravitative force acting on the body will vary directly with the sum of its units, *i. e.*, with its mass.

The spherical shadow which I have pictured as the field of influence of each atom or material unit implies that the atom has caused, principally in its immediate neighborhood, a diminution of the ether's energy. Let us further imagine this subtracted energy resident in the atom as kinetic energy of translation in many paths, almost infinitesimally short and in every direction, but without collisions because neighboring atoms follow *very* nearly parallel paths. We may then picture the collective atoms or molecules of matter buffeted about in every direction by the ether waves in which they are entangled, like a suspended precipitate in turbulent water.

Each atom or molecule may be regarded as a center of activity due to its kinetic energy of translation, with continual absorption and restitution of the ether's energy, normally equal in amount. The manner in which this molecular activity maintains, in effect, the supposed spherical shadow requires explanation which I shall attempt in a future paper.

Of the several components into which the composite motion of each atom can be resolved, that one lying in the direction of an attracting body will be the greatest, because the waves from that direction, being partially intercepted by the attracting body, are weakest; and the atom will be *pushed* in that direction by the superior waves behind it. If free to fall, the atom will continually absorb more energy from the stronger waves behind it than it restores to the weaker waves in front, and will thus acquire additional kinetic energy of translation in the line of fall, measured directly by the number of waves involved, *i. e.,* by the distance moved. Conversely, if the atom be forced away from the attracting body, restitution of energy will exceed absorption, and the energy expended in moving the atom against attraction will be transferred to the ether.

It will be seen that gravitation is a *push* toward the attracting body, and not a pull. It is clear, also, that the velocity which a falling body can acquire tends asymptotically to a limit, which is the velocity of the ether waves which push it—the velocity of light, if transverse waves are involved.

I have already intimated that any kind of ether waves capable of imparting motion (not internal vibration) to the atoms of matter will fulfil the requirements of my theory, but have thus far discussed only transverse waves.

Let us now consider longitudinal waves—waves of compression and rarefaction, like sound waves in air and in elastic liquids and solids. The "spherical shadow" conception which I have employed in connection with transverse waves applies equally well here.

So far as I am aware, longitudinal waves in the ether are unknown; but that such waves have not been observed is not convincing argument that they do not exist.

Assuming then that some, or perhaps much, of the intrinsic energy of the ether is embodied in longitudinal waves, we have only to find some motive action of such waves on atoms of matter to account for gravitation. Adequate motive connection may perhaps be effected by the locally alternating flow and ebb—acceleration and retardation of the ether in which the atoms are enmeshed, incident to its wave motion. We have ample reason for believing that the ether does obtain a grip of some sort on the atoms of an accelerating (falling) body, and a retarding (rising) body, from which it follows that accelerating and retarding ether, as in a wave of compression, must grip a comparatively stationary atom.

Certain facts of astronomy apparently require that gravitational attraction between bodies, however distant from each other, must, in effect, be instantaneous; that is to say, the line of apparent attraction between them is a straight line joining their centers. I believe my theory meets this condition, but I shall reserve discussion of the point for a future paper.

I feel much diffidence in presenting the foregoing rough draft of a theory of gravitation; but I can not avoid the belief that it contains some germs of truth, perhaps the real key to the great mystery, though, if this be true, I have, no doubt, used the key clumsily and imperfectly.

If the ether-wave theory of gravitation is, in the main, the true one, it offers some hope of experimental verification. Provided the waves are of one principal frequency, or even of several, we may find something, doubtless of molecular magnitude only, which will oscillate in unison with them so that resonance can occasionally be established and a cumulative effect obtained sufficient to manifest itself as heat.

In searching for some natural phenomenon of this nature, I thought of the thermal condition of the upper atmosphere as a possible case. The mean molecular velocity of a gas at some temperature, in connection with the

mean free path of its molecules at some particular pressure or pressures, may possibly afford the necessary conditions for fortuitous resonance, with development of some slight amount of heat by the increased violence of inter-molecular collisions. I have done much experimental work on these lines during the past year, but, notwithstanding refinement of method and manipulation, the results have thus far been unsatisfactory. The work is still in progress, however, and investigation of other phenomena is contemplated.

CHARLES F. BRUSH
CLEVELAND, OHIO

AMERICAN SOCIETY OF ZOOLOGISTS EASTERN BRANCH

THE Eastern Branch of the American Society of Zoologists held its annual meeting on December 27–30, 1910, inclusive, at Cornell University, Ithaca, N. Y., in conjunction with the American Society of Naturalists, the American Association of Anatomists and the Society of American Bacteriologists.

The following officers were elected:

President—H. V. Wilson, University of North Carolina.

Vice-president—H. E. Crampton, Columbia University.

Secretary-treasurer—Raymond Pearl, Maine Agricultural Experiment Station.

Additional Member of Executive Committee—R. G. Harrison, Yale University.

The following persons were elected members of the American Society of Zoologists at this meeting: Dr. Alice M. Boring, University of Maine; Dr. O. A. Johannsen, Maine Agricultural Experiment Station; Professor R. E. Sheldon, University of Pittsburgh; Professor A. E. Lambert, State Normal School, Framingham, Mass.; Dr. R. C. Schiedt, Franklin and Marshall College; Dr. Sergius Morgulis, Harvard University; Professor E. W. Gudger, State Normal College, Greensboro, N. C.; Dr. A. M. Banta, Carnegie Institution, Station for Experimental Evolution; Professor G. G. Scott, College of the City of New York.

A committee was appointed to prepare a resolution on the death of Professor C. O. Whitman. This resolution will be published in a later number of SCIENCE.

The following papers were presented at the meeting, either in full, or by title:

The Spermatogenesis of the Opossum: H. E. JORDAN, University of Virginia.

An accessory chromosome and chondriosomes are the structures of special interest. Metaphase plates of dividing spermatogonia contain 17 rod-shaped chromosomes (diploid group; 16 autosomes, 1 monosome). A chromatin- (chromosome) nucleolus is present during the growth period (including synizesis and synapsis) invariably at that point near the nuclear membrane next the centrosphere. The first numerical reduction results from a pairing end to end (telosynapsis) of the 16 autosomes. The haploid chromosome group thus contains 9, the accessory recognizable by its larger size and bipartite form. During metakinesis (reduction division) the accessory chromosome passes undivided, and in advance of the ordinary chromosomes, to one pole. Two types of secondary spermatocytes result: one with 9, the other with 8 chromosomes. During the brief resting phase one type has a chromatin-nucleolus, the other lacks this structure. A second numerical reduction has occurred—a phenomenon previously described by Bardeleben (1898) for man, and quite recently by Guyer for certain birds—giving rise to hemioid groups containing 5 and 4 chromosomes, respectively. The ensuing division is equational. In the early spermatid-phase a resolution takes place giving 9 and 8 chromosomes, respectively. A dimorphism of spermatozoa thus results. Chondriosomes (mitochondria) appear in early postsynaptic stages (probably as chromidia passed out of the nucleus). A direct continuity is demonstrable between the chondriosomes and the spiral filament of the middle piece of the spermatozoon. No twin spermatozoa, such as Selenka described in the vas deferens of the opossum, appear in the testes studied.

The complete paper will appear in the *Archiv für Zellforschung.*

The Germ Cell Determinants of Beetles' Eggs: ROBERT W. HEGNER, University of Michigan.

This report is based on the results of experiments in killing parts of the eggs of some chrysomelid beetles. The posterior ends of freshly-laid eggs contain a disc-shaped mass of granules that stain like chromatin. These granules are taken up by the cleavage products that encounter them; these cleavage products later become germ cells. For this reason the granules have been called germ-cell determinants. When the posterior ends of freshly-laid eggs are killed with a hot needle, thus preventing the granules from taking part in

development, the resulting embryos do not develop germ cells. This evidence strengthens the hypothesis that these granules are really germ-cell determinants.

The complete paper will be published in the *Biological Bulletin*.

Heterochromosomes in Mosquitoes: N. M. STEVENS, Bryn Mawr College.

An unequal pair of heterochromosomes is found in the male germ cells of *Anopheles punctipennis*, and the heterochromosome differentiation can be seen in the resting spermatogonia and in the spermatids as well as in the spermatocytes. In *Culex pipiens, Culex tarsalis* and *Theobaldia incidens* no such differentiation of heterochromosomes is present. As the sex-determining mechanism is without doubt the same in the several species of mosquitoes, it is evident that heterochromosome differentiation is not a necessary factor in the determination of sex. In *Culex* and *Theobaldia* we certainly can not say two X chromosomes give a female and one X chromosome or an X and a Y chromosome give a male, for there are no X or Y chromosomes. However, it is evident that, although the heterochromosome differentiation may have nothing to do with sex-determination, the sex-determining factors must be closely correlated with it, and may in some cases be located in the heterochromosomes. The importance of a study of the origin and history of the heterochromosomes apart from the idea of sex-determination is urged. The complete paper will be published in the *Biological Bulletin*.

Origin and Significance of Mitochondria: T. H. MONTGOMERY, JR., University of Pennsylvania. (An excerpt from a paper to be published in the *Journal of Morphology* on the spermatogenesis of *Euschistus*.)

The mitochondria undergo their chief growth and multiplication in the growth period of the germ cells, and are few or absent in the spermatogonia. Their mode of division in the reduction mitoses is irregular, and they become divided by the cell constriction. They do not arise as eliminated chromatin nor from the allosomes, but apparently as a chemical interaction of nucleus and idiosome. They represent part of the epigenetic history of the germ cells, and the chromosomes the preformistic.

The Method of Cell Division in Moniezia: A. RICHARDS, Princeton University. (Introduced by Ulric Dahlgren.)

A reinvestigation of the development of the female sex products of *Moniezia* has not substantiated the claim of Child that amitosis has a regular place in this development. Amitotic divisions were found at no stage in the growth of these products. On the other hand, mitoses occur at all stages in the development, although more rarely in the early oogenesis than might have been expected. Probably also periodicity of mitotic divisions is one reason for the relative scarcity of spindles at this stage. Cleavage divisions are positively mitotic for the spindle in each cell generation may be found. The complete paper will appear in the *Biological Bulletin*.

The Relation between the Formation of the Fertilization Membrane and the Initiation of the Development of the Echinoderm Egg: J. F. MCCLENDON, Cornell University Medical College.

Loeb observed that the sea urchin's egg may develop without the formation of a fertilization membrane, and I have confirmed this observation, and shown that Harvey is very probably wrong in his supposition that this is a case of failure i "pushing out" of the membrane. Therefore I concluded that "membrane formation" is not essential for the segmentation of the egg, although by furnishing protection it may insure the development of the embryo.

Loeb postulated that an osmotically active colloid exists in the unfertilized egg, but is so covered with lipoids that it does not absorb water until it is squeezed out or otherwise exposed at the surface of the egg, at the beginning of development (when it fills the so-called "perivitelline space"). I have shown[1] that this substance bears a positive charge (is basic) since it migrates toward the kathode when an electric current is passed through sea water containing the fertilized egg.

The unfertilized egg is imbedded in a mass of jelly which is probably mucin. This jelly bears a negative charge (is acid) since it combines with color bases.

When the positively charged colloid is exposed at the surface and comes in contact with the negatively charged jelly, the two mutually precipitate at their surface of contact, thus forming the fertilization membrane. But if all of the jelly is washed off of the egg before the latter is caused to develop, no fertilization membrane is formed (as I have observed) because no two oppo-

[1] *Am. Jour. Physiol.*, 1910, XXVII., 240.

sitely charged colloids are brought in contact, but the basic colloid may with difficulty be seen as a refractive layer, which has been mistaken for a poorly developed "fertilization membrane."

The increased permeability of the egg surface, which releases the basic colloid, is one of the prerequisites to the increased oxidation of the developing egg, and in this way membrane formation and development are induced by the same change.

Evidence for the Transmission of the "Wound Stimulus" to Underlying Tissues and its Relation to Regeneration: J. F. McClendon, Cornell University Medical College.

The "current of injury" produced by the negative electric potential of a wounded surface is common to animal and plant tissues. The wounded cell acts as an electric generator and a current flows through neighboring cells. If a current is passed through living tissue, which is subsequently fixed and stained, basophile substances will be found displaced toward the anode. In sections of tissue adjacent to a wound the extent of the current is indicated by the displacement of basophile granules. The current affects first the cells in contact with the wounded cells, then extends in some directions more than others. Electric currents (currents of growth) continue for many days after the wound has healed. Since electric currents cause sea-urchin eggs and tissue cells to divide and proliferate, probably these bioelectric currents constitute the so-called "formative stimulus" of regeneration.

Maturing Reagents and those Inducing Segmentation in Artificial Parthenogenesis: Max Withrow Morse, Trinity College.

An extended series of experiments, continued throughout the summer at the Harpswell Laboratory upon the eggs of *Cerebratulus*, demonstrated that those reagents which induced maturation of this egg, would not cause development to proceed farther through the segmentation stages and indeed evidence for an inhibiting action to segmentation on the part of the solutions used in causing the egg to throw off the polar bodies, was obtained. The glucoside saponin, dibasic acids such as oxalic, hydrochloric and tartaric acid were used successfully to mature the egg, but no subsequent application of these reagents caused segmentation to proceed. It was found, however, that if every trace of these solutions was removed by careful washing and the eggs placed in a CO_2-sea-water solution, with a concentration of approximately 0.19 g. to the 100 g. sea water, segmentation proceeded. However, no method was found whereby the eggs could be carried beyond the later segmentation stages. Loeb and others have observed this antagonistic action of maturing and segmentation-producing reagents in other forms, and in such cases, as in the present one, the reactions are not reversible; CO_2 will not cause maturation. The experiments were checked against temperature, salinity, alkalinity and such external factors as might modify results.

Newly Found Odonate Larvæ of Special Interest from Costa Rica: P. P. Calvert, University of Pennsylvania.

The larva of *Cora* possesses two-branched mandibles, and paired ventral tracheal gills (= modified legs?) on abdominal segments 2–7, in addition to three thick caudal tracheal gills. A detailed account has appeared in *Entomological News* for February, 1911.

The larva of *Mecistogaster modestus* lives in the rain water between the leaf-bases of arboricolous bromeliads. The remarkable increase in length at transformation, from the larva measuring 24 mm. long to the adult 82 mm. long, occupying about one and a half hours, was illustrated by a series of lantern slides from life. The full description will probably be published in the journal quoted above.

The Chondrocranium of Eumeces (preliminary report): Edward L. Rice, Ohio Wesleyan University.

Preliminary comparison with chondrocranium of *Lacerta* as described by Gaupp. For most part these skulls are very similar, but with some striking differences. Particularly striking is enormous size in *Eumeces* of pars cochlearis of otic capsule. This extends far down into region of basal plate, displacing facialis foramen relatively upward between the two parts of otic capsule.

In no single stage is lateral wall of temporal region so complete as in *Lacerta*, although all the same elements may be recognized in a comparison of various stages, some parts being in regression while others are progressively developing. In earliest stages tænia parietalis media extends upward and backward to unite with tænia marginalis, thus dividing fenestra prootica into upper and lower portions, latter furnishing exit for trigeminal nerve.

Cartilage of interorbital septum continuous in younger specimens; progressively fenestrated in later stages.

Nasal capsule in general less complete than in

Lacerta; but large fenestra lateralis nasi, emphasized by Gaupp, wanting in all stages.

Considerable individual variation in arrangement of nerve foramina. Without reference to age, hypoglossus foramina may be either three or two on each side, or three on one side and two on other. Also irregularities in course of abducens.

Discussion of columella auris deferred.

The Taxonomic Value of the Brain: B. G. WILDER, Cornell University.

That the brain presents distinctive characters in certain teleostean families was claimed by L. Agassiz in 1844.[2] The taxonomic value of encephalic characters has been maintained at different times by Owen, Gill and the writer. Nevertheless, recent revisions of various groups do not even mention the brain, and four years ago a review[3] by a former secretary and vice-president of the zoologic section of the American Association for the Advancement of Science declares that the study of the nervous system has no value from any other point of view than that of function. The present paper mentioned instances of generally accepted groupings that might have been based upon the brain alone; recalled malassignments (*e. g.*, of the Sirenia with the Cetacea, of ganoids and selachians as *Palæichthyes*) that might have been averted by due consideration of the brain; and held that such consideration forbade the association of ganoids and dipnoans as a "ganoid-dipnoan phylum," warranted the separation of lampreys and hags as coordinate groups of class grade, and showed that *Pristiophorus* is a primitive type not nearly related to *Pristis*. Standard treatises evince indifference toward the brain or ignorance of it, and are open to criticisms comparable with those in the *American Journal of Science*, Vol. 20, July, 1880, p. 76; likewise the commingling of constant and peculiar characters with others not really distinctive, as deprecated in 1885[4] and in 1894.[5]

The Histology of the Oviduct of the Domestic Fowl: FRANK M. SURFACE, Kentucky Agricultural Experiment Station.

This paper will be published in the Annual Report of the Maine Agricultural Experiment Station for 1911.

[2] *Bull. Soc. Sci. Nat. Neuchatél*, December 4, p. 147.

[3] SCIENCE, Vol. XXIV., p. 846.

[4] SCIENCE, Vol. VI., p. 223, September 11.

[5] *Asso. Amer. Anat., Proceedings*, 7th session, p. 19.

The Lampreys of the Cayuga Lake Basin: Fate of Lampreys after Spawning; Non-parasitism of the Brook Lamprey: SIMON H. GAGE, Cornell University.

After spawning, lake lampreys were put into wire cages with live cat-fish. The cages were kept in the running water of the stream where they spawned, and in the lake water at the Limnological Station. The spent lampreys never fed upon the cat-fish, and soon died. Dead bodies of lampreys were found in great abundance along the stream. The discovery was made that the notochord is very persistent, enduring in full perfection after all the rest of the animal had decayed. There seems great probability that under favorable conditions the notochords might become fossilized; if so they would puzzle the paleontologist.

The non-parasitism of the brook lamprey was first reported by me in 1898. The demonstration has been repeated during the years 1907, 1908 and 1909, by keeping the transforming animals over winter in an aquarium. They live under the sand like the ammocœte, and only emerge in the spring when their sexual products are ripe. Lake lampreys when they transform attack fish with great ferocity, and suck their blood, but the brook lamprey never attacks fish under the most favorable conditions. When they emerge from the sand they lay their eggs and die. From their structural adaptation to parasitism it is believed that they were once parasitic, but have lost that habit.

Protective Coloration in Poultry: RAYMOND PEARL, Maine Agricultural Experiment Station.

This paper has appeared in the *American Naturalist* for February, 1911, under the title: "Data on the Relative Conspicuousness of Barred and Self-colored Fowls."

Adaptive Color Changes in Flounders: F. B. SUMNER, U. S. Bureau of Fisheries.

The author described the results of some inquiries, conducted at Naples and Woods Hole, into the relation between the visible background and the color-shade and pigment-pattern assumed by the fish.

Sense of Smell in Selachians: R. E. SHELDON, University of Pittsburgh. (Introduced by S. H. Gage.)

1. A current of water, caused by the respiratory movements, and augmented by the forward motion of the fish in swimming, courses through the nasal capsules of the dogfish.

2. By this means substances in solution in the water come in contact with the olfactory mucous membrane.

3. Dogfish recognize and determine the location of food substances through a chemical sense.

4. This power is lost when the olfactory capsules are filled with loose cotton. It is regained when the cotton is removed.

5. The plugging of one nostril only does not seriously affect this power.

6. The dogfish obtains its food almost, if not entirely, through the use of the sense of smell.

7. The selachians possess a true sense of smell, comparable to that of the terrestrial vertebrates.

The complete paper will appear in the *Journal of Experimental Zoology*.

Habits and Reactions of the Ciliate, Lacrymaria: S. O. Mast, Goucher College.

Lacrymaria is usually found with the body well concealed in débris while the head stretches out in all directions and moves rapidly about forward and backward and from side to side apparently exploring every nook and crevice within its reach which often extends to a distance equal to eight times the length of the body. In this way the creature captures other organisms on which it feeds. It never swallows dead particles, showing that it exercises the power of selection in the process of feeding.

It is usually assumed that the movements are regulated by the contraction of tissue in the neck and body, but this is not true. The head is not thrust out; it is pulled out. Nearly all of its actions are due to the activity of a band of powerful cilia which extends around the mouth. Thus *Lacrymaria* is much like an organism composed of two independent parts united by means of an extremely elastic substance far more elastic than any known lifeless material. When the neck is fully extended it is frequently fifty times as long as when contracted.

There is no indication of orientation in *Lacrymaria* under any condition. It does not respond to light. None of its reactions fulfil the demands of any of Loeb's definitions of tropisms. The movement of the entire organism is almost entirely regulated by the reactions of the head, and the direction of movement of the head is regulated almost entirely by internal factors; it is practically independent of the location of the stimulus. We assume that all of the reactions in this animal are definitely determined by physico-chemical processes, but whether they are or not has by no means been demonstrated.

The complete paper will probably be published in the *Journal of Animal Behavior*.

The Reaction System of the Flagellate, Peranema: S. O. Mast, Goucher College.

Under natural conditions *Peranema* rarely swims. It ordinarily moves in contact with the substratum without rotating. Only the tip of the flagellum is active. This is bent at right angles to the rest and strikes strongly backward. When the organism is stimulated no matter by what means or at what point it responds by turning the anterior end with the flagellum sharply, always toward the same side. Then it straightens out and proceeds on a new course usually at an angle of about 90° with the old. If the stimulus does not cease the same reaction is repeated until it does. This is the only method which *Peranema* has for changing its course in its usual method of locomotion. The action of the flagellum is not functional in this. If strongly and repeatedly stimulated a larger portion of the flagellum may be brought into action and there may be peristaltic contractions in the body. Both of these processes affect the rate of motion but not the direction. The direction of turning is entirely dependent upon internal factors; it is independent of the location of the stimulus on the body. The direction of movement is not definitely determined by external factors. It is dependent upon "trial" positions which are assumed by turning the anterior end in response to a stimulus; only such as do away with the action of the stimulus are followed up.

The complete paper will probably be published in the *Journal of Animal Behavior*.

The Behavior of Certain Arthropods in Relation to Color Environment: A. S. Pearse, University of Michigan.

As a result of experiments with crayfishes, caddis-fly larvæ, spider-crabs and spiders the conclusion is drawn that protectively colored arthropods do not select an environment similar to their own; at least they do not make such selection on account of color.

The complete paper will be published in the *Journal of Animal Behavior*, Vol. I., pp. 79–110.

On the Regenerative Power of the Dissociated Cells in Hydroids: H. V. Wilson, University of North Carolina.

It had been shown that when certain sponges are broken up into their constituent cells, the cells will reunite and form masses capable of differentiation into functional sponges. Two hydroids, *Pennaria* and *Eudendrium*, were found to possess the same power. The dissociated cells

and small cell aggregates fuse and give rise to lumps, which become smooth and secrete a perisarc. Their size and shape are within control. Such bodies are solid masses in which cell limits exist, although the structure may be in part syncytial. They show at first but little change and are subject to great mortality. After two or three days many throw out one or more cylindrical outgrowths in which ectoderm and entoderm are differentiated. In the case of some of these, growth and differentiation continue until the end of the outgrowth is transformed into a perfect hydranth.

Masses resembling those just described were obtained in a similar way from an alcyonarian, *Leptogorgia*, and when the immature gonad of *Asterias* was broken up, the cells quickly fused, forming lumps and plates. Probably the power to fuse resident in the cells and cell aggregates into which a body (in the case of lower metazoan) or tissue (in case of higher form) may be broken up, is wide spread. What degrees of regenerative power may be resident in such masses is a matter for investigation. (Paper to appear in the *Journal of Experimental Zoology.*)

The Problem of Form in Hydra: HERBERT W. RAND, Harvard University.

In the problem of form as presented in ontogenesis we have progressed so far as to be able to state confidently that the essential form-determining factors are within the organism itself. We must now discover to what extent and where in the organism these formative agencies are localized. The line of attack upon this problem lies in experiments involving various derangements of the normal form of relatively simple organisms. The available morphogenetic data upon *Hydra* probably exceed in quantity and diversity those upon any other equally simple organism.

An examination of the total evidence afforded by *Hydra* leads to the conclusion that in the normal adult the peristome is the seat of some peculiarity by virtue of which that region exercises formative control over column substance, whether it be substance of the column to which the peristome originally belonged, or of any other column with which the peristome material comes into relation by transplantation. When a piece of column regenerates a head, this form-controlling condition is established in the prospective peristome material in advance of the visible formation of oral organs and as a prerequisite of it (Browne, 1909). *This localizing of the formative agencies is a function of the whole of the regenerating piece.* In the normal hydra, therefore, heads are not, in any direct sense, preformed at various levels of the column.

In graft-compounds having two or more heads, the regulatory changes may, without exception, be interpreted as dependent upon competition of the several peristomes for control of the column substance.

The phenomena of regeneration and regulation fairly compel the assumption of the existence of a specific formative force-system for which those particular chemical complexes which constitute hydra substance serve as the vehicle; and, together with the phenomena of polarity, they afford ground for some conception of the distribution and mode of operation of such a force-system. (A full treatment of the subject is being prepared for publication.)

The Proportion of Male-producers in Hydatina senta as Affected by External and Internal Factors: A. FRANKLIN SHULL, Columbia University.

The proportion of male-producers in the rotifer *Hydatina senta* can be reduced by rearing the animals in weak solutions of urea, several ammonium compounds, beef extract or creatin. Since some of these substances exist in the food cultures used in the experiments, starvation may appear to increase the proportion of male-producers because smaller quantities of these substances are administered with the food.

Different pure lines of rotifers obtained from widely separated localities yielded different proportions of male-producers when reared under the same conditions; this indicates the existence of an internal factor that plays a rôle in determining the proportion of male-producers. When individuals belonging to distinct pure lines were crossed, the offspring gave rise to pure lines (F_1) yielding more male-producers than either parent line. When a member of one of these F_1 lines was crossed with an individual from one of the parent lines, the zygote gave rise to a line (F_2) producing a proportion of male-producers intermediate between those of its two parent lines. The explanation of these results is not yet clear. (To be published in the *Journal of Experimental Zoology.*)

Evolution of Hectocotylism among Cephalopods: G. A. DREW, University of Maine.

The paper read will be included in a paper bearing the following title: "Sexual Activities of the Squid, *Loligo pealii* (Les.)—1. Copulation, Egg-laying and Fertilization." It is to be pub-

lished in the *Journal of Morphology* during the year of 1911.

The Idiochromosomes in Ascaris felis: C. L. EDWARDS, 661 East 170th St., New York City.

Effect of Conjugation on the Stock in Paramecium: H. S. JENNINGS, Johns Hopkins University.

The Organs of Equilibration in Pelecypod Molluscs: ULRIC DAHLGREN, Princeton University.

The Anatomical Basis of Mulatto Color: H. E. JORDAN, University of Virginia.

This paper will appear in the *American Naturalist.*

Variation in the Embryos of the Hagfish, Homea (Bdellostoma) stouti: BASHFORD DEAN, Columbia University.

A New Phase of the Question of the Irritability of the Skin of Vertebrates to Chemical Stimuli: G. E. COGHILL, Denison University.

The Comparative Toxicity of a Series of Electrolytic and Non-electrolytic Compounds with Respect to Fundulus heteroclitus: R. E. SHELDON, University of Pittsburgh. (Introduced by S. H. Gage.)

The Senses, Courtship and Mating in Tarantulas: A. PETRUNKEVITCH, Yale University.

A Case of Regeneration in Tarantulas: A. PETRUNKEVITCH, Yale University.

The Origin and Heredity of Four Wing Mutations in Drosophila: T. H. MORGAN, Columbia University.

The Heredity of Red Eyes, White Eyes and Pink Eyes in Drosophila: T. H. MORGAN, Columbia University.

The University of Michigan Biological Station: A. S. PEASE, University of Michigan.

In addition to the papers read the following exhibits and demonstrations were presented:

Specimens of the 2100th Generation of Paramecium aurelia, attained without Conjugation or Artificial Stimulation: L. L. WOODRUFF, Yale University. (Presented by title only.)

On the Senses, Courtship and Mating in Tarantulas—Regeneration in Tarantulas: A. PETRUNKEVITCH, Yale University.

Inheritance of Color in Colias philodice: J. H. GEROULD, Dartmouth College.

Regeneration in Hydroids: H. V. Wilson, University of North Carolina.

RAYMOND PEARL,
Secretary

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

SECTION D—MINNEAPOLIS MEETING

THE section held its meetings on Thursday and Friday of convocation week. The Thursday morning session was devoted to the routine business connected with election of officers and fellows and to a program of papers, ten in number, of which eight were devoted to road and highway problems. These papers were interesting and valuable contributions and should have been heard by a much larger audience than were in attendance.

Thursday afternoon the section met in joint session with Section B and listened to the address of retiring Vice-president Hayford of Section D on the subject "The Relation of Isostasy to Geodesy, Geology and Geophysics" and to that of Vice-president L. A. Bauer, entitled, "The Broader Aspects of Research in Terrestrial Magnetism." Both addresses have been published in full in SCIENCE and are well worth reading by those who heard them at the meeting as well as by others interested in the subjects. These sections have ever been fortunate in their vice-presidential addresses.

Thursday evening was given over to a dinner and smoker for the members of Sections A, B and D and the Chicago Section of the American Mathematical Society. The dinner was served by the Commercial Club of Minneapolis. The postprandial remarks were enjoyed and enjoyable.

Friday morning Section D held a session devoted to Aeronautics at which nine papers were read by or for the authors. Quite appropriately the program opened with an appreciation of Dr. Octave Chanute written by James Means, of Boston, and presented by Professor A. Lawrence Rotch, vice-president of the section. This contribution will be printed in full in SCIENCE. On Friday afternoon, Sections A, D and the Chicago Section of the American Mathematical Society met in joint session for a symposium on engineering-mathematics, the same being a discussion of a preliminary report on the subject prepared by a committee appointed at the time of the Chicago meeting of the American Association for the Advancement of Science. Printed copies of the report had been prepared by the chairman of the committee, Professor E. V. Huntington, of Howard University, who opened the discussion, which was continued with spirit during the entire session with profit to all present.

Professor A. Lawrence Rotch, vice-president of

the association and chairman of Section D, presided at all meetings of the section and at the joint session of Sections B and D.

The officers for the Washington meeting of the section are as follows:

Vice-president—C. S. Howe, Case School of Applied Science.

Retiring Vice-president—A. Lawrence Rotch, Blue Hill Observatory.

Secretary—G. W. Bissell, Michigan Agricultural College.

Member of the Council—A. F. Zahm, Washington, D. C.

Member of General Committee—O. F. Marvin, University of Kansas.

Sectional Committee—J. E. Boyd, Ohio State University; A. H. Blanchard, Brown University; C. M. Woodward, Washington University; W. J. Humphreys, Mount Weather Observatory; G. O. Squier, U. S. A.

Herewith are titles and abstracts of the papers presented:

The Amount of Stream-flow in the Northern Prairies: E. F. CHANDLER, University of North Dakota.

In the prairie regions of the northern United States, there have been until lately no stream-flow records available. Within the past decade, the U. S. Geological Survey and other agencies have maintained fairly extensive records here. It is now known that in this region the stream-flow or "run-off" is far less than formerly supposed. In North Dakota, as a particular example, it is seen that some former estimates were as much as 500 per cent. in error, for the average annual run-off is less than one inch and the total of a single year is frequently less than one half inch.

The run-off from any drainage area depends upon its topography, geological structure, etc., and upon its total rain-fall and the seasonal distribution and intensity of the rains. But where the run-off is as small as here considered, it is very improper and misleading to follow the frequent custom of speaking of the run-off as a *percentage* of the rainfall.

The only basis upon which any reasonable estimates can be made is a comprehensive set of rain-fall and stream-flow records extending through a long period of years. Rain-fall records alone are not sufficient; this statement applies especially to regions where the stream-flow is only a small percentage of the rainfall.

Consequence of Solution of Air in the Hydraulic Air Compressor: F. W. MCNAIR and GEO. A. KOENIG, Houghton, Mich.

Brief description of the Taylor compressor at the Victoria Mine, difficulty with lights in the mine, plan of the mine, conditions due to "compressor air" as affecting lights and men, comment on efficiency of compressor, desirability of further investigation; being a brief résumé of the results of an investigation made in the spring of 1907 by the authors.

A Comparison of English and American Traffic Regulations: A. H. BLANCHARD, Brown University, and H. B. DROWNE, Providence, R. I.

From the standpoint of the highway engineer the following regulations should be made a part of the laws relative to the use of highways by various kinds of traffic. The proposed regulations are based on conclusions arrived at after a careful consideration of the effect of various classes of traffic on road surfaces as exemplified by American and European practice.

1. All horse-drawn vehicles shall be equipped with a light or lights.

2. All vehicles, either horse-drawn or motor-driven, having iron tires and using the improved state roads shall be provided with tires of widths such that for a 2-foot wheel 500 pounds shall be the maximum pressure per linear inch of width per wheel, but an additional pressure of 30 pounds per inch shall be allowed for each additional 3 inches in diameter. The maximum width of tires for horse-drawn vehicles shall be 6 inches. All iron tires must be smooth. The width out to out of all classes of vehicles, including the superimposed load, shall not exceed 8 feet.

3. The maximum speed of motor cars on all highways shall not be greater than is reasonable and proper, having regard to all classes of traffic and local environment.

4. Vehicles shall be so constructed that the driver or operator shall sit on the left rather than on the right.

5. Motor cars (including traction engines) hauling trailers shall keep to the right side of the center of the road. No more than three trailers shall be allowed in any train. Pneumatic tires or tires made of soft or elastic material shall not be equipped with chains, metal studs or some other non-skidding device of this character except in the case of passenger vehicles carrying not more than seven persons.

6. The intensity of powerful acetylene lights on motor vehicles, including those running on rails, shall be diminished on the approach of other vehicles.

7. Sign posts shall be erected by the state highway departments to give notice of points of danger and to give information as to the route of the road.

Relation between Modern Traffic and the Alignment and Profile in Highway Design: H. B. Drowne, Providence, R. I.

Preliminary to designing a road, a careful study should be made of the existing traffic conditions and those to be expected, since there are several features in the design that depend upon whether a road is to serve only a horse-drawn vehicle traffic, a combination horse-drawn vehicle and motor-car traffic or a motor-car traffic alone. Wider roads are required where there is much motor-car traffic and on important roads a minimum width of twenty feet. is advocated. Also the transverse slope or crown of the road should be made from one fourth inch to one half inch per foot. Sharp curves are not detrimental to a road that takes mostly a horse-drawn vehicle traffic, but if a heavy motor-car traffic is expected all bad curves should be eliminated or reduced as much as possible, since they are not only expensive to maintain but are also extremely dangerous. Moreover, the curves should have a one-way slope up from the inside edge, as this will distribute the wear more evenly over the entire width of the road. There is no need of reducing the maximum grades now in common use on the improved roads.

The Present Status of the Use of Bituminous Materials in the Construction and Maintenance of Roads in the United States: A. H. Blanchard, Brown University.

As an indication of the development of the use of bituminous materials in road construction and maintenance, statistics are submitted based on the work accomplished by the state highway departments of Maine, Maryland, Massachusetts, New Hampshire, New Jersey, New York and Rhode Island. The total amounts of bituminous work in the various years are as follows: 1908, 350,000 sq. yds.; 1909, 7,750,000 sq. yds.; 1910, 18,000,000 sq. yds.

The most notable developments have been: (1) In the increased use of liquid asphalt and heavy asphaltic oils for surface treatment as is shown by the following figures, 1908, 208,000 sq. yds.; 1910, 2,434,000 sq. yds. (2) In the increased use of light oil as a dust layer, as is indicated by its non-employment in 1908, while 9,825,000 sq. yds. were treated in 1910. (3) The extensive use of asphalts, solid at ordinary temperatures, or combinations of asphalt and tar in connection with the penetration method of constructing bituminous pavements, the total square yards built in 1910 being 4,400,000 sq. yds., while no work of this character was done in 1908. (4) The increase in the use of liquid asphalts and heavy asphaltic oils in the construction of bituminous pavements by the mixing method, as is shown by the following figures, 1908, 3,000 sq. yds.; 1909, 151,000 sq. yds., and in 1910, 335,000 sq. yds.

Also attention is called to the more general use of refined tar in place of crude tar, the increased use of mechanical distributors both in the surface treatment of old roads and the construction of bituminous pavements by the penetration method and the introduction of mixing machines in connection with the construction of bituminous pavements by the mixing method on state highway work.

Certain Considerations affecting the Selection of Bituminous and Mineral Matter for Road Construction and Surface Treatment: J. C. Travilla, St. Louis, Mo.

Highway engineers are giving much attention to the investigation of bitumens and to the development of specifications for bituminous materials, but in the opinion of the writer insufficient attention is paid to the study of the mineral aggregate. The life of an asphalt or bitulithic pavement has been shown to depend upon the character of the aggregate to a great extent, and in the case of bituminous concrete or a bituminous wearing surface the same is true.

In general mineral dust is as objectionable as moisture in the construction of bituminous roads.

The wearing qualities of the mineral matter employed affect the life of a road and also the form of construction. In the case of a very soft stone better results ensue when the larger stone is placed on top.

The writer's experience proves to him that resiliency in the surface is of importance in lengthening the life of a road or pavement. Bitumens should, therefore, not be brittle at low temperatures nor should they be too soft at high temperatures. St. Louis has specifications governing the special adaptability and the methods of application for surface treatment of four grades of oil.

Care should be taken to keep oiled streets clean. The oiling of all road intersections is important in this connection.

Need of and the Opportunity for Technically-trained Men in Highway Work: A. N. Johnson, Springfield, Ill.

This paper reviews briefly the history of road

building in the United States, noting that from the time of the advent of railroad construction, little or no attention was given by engineers to road work until about 1890, when some of the eastern states took up the question of road improvement from state-wide view points, since which time large sums of money have been appropriated by different states for the systematic study and construction of roads; that this work is now demanding the service of trained men and the people generally are realizing the necessity of such service.

In presenting the paper, the writer illustrated with lantern slides some of the conditions found in highway work in Illinois and the methods of the road and bridge construction undertaken by the Illinois Highway Commission which would be representative of the conditions and methods applicable to a wide area of the Mississippi Valley region.

Methods of Taking Traffic Census on Highways: A. H. BLANCHARD and I. W. PATTERSON, Providence, R. I.

The value of statistics relative to traffic on highways, taken previous to their construction, is generally admitted; but the methods to be employed in the securing of such statistics have not been discussed in the technical press except relative to the classification of traffic.

In view of the fact of their close relation to the wear of a road surface, the following elements of classification should be noted: differentiation between horse-drawn and motor-car traffic, distinction between pleasure and commercial traffic, subdivision of commercial traffic into loaded and unloaded vehicles, weight per linear inch of tire of commercial traffic, number of horses drawing vehicle, weight and speed of motor cars and abnormal local traffic of extraordinary character. Numerous highway supervisory bodies have drawn up forms which include the above important items more or less completely.

The methods of securing traffic data are of extreme importance. The methods where two extremes of time (one year and one month) are available for traffic census are discussed. The following three methods applicable in both cases are considered: (1) observations upon single days at regular intervals; (2) observations through a period of several consecutive days at intervals; (3) observations covering a period of three days, including Saturday and Sunday, at intervals. The last-named method appears from the standpoint of economy of labor, probable clemency of weather (weather conditions at Providence, R. I., considered in each case for a period of five years), and results obtained to be the most practical method for general use.

The Present Status of the Relationship between Laboratory Tests and the Use of Road Materials: W. W. CROSBY, Baltimore, Md.

The science underlying the art of roadmaking is obtaining recognition. For its development, laboratory tests and records of experience are necessary. Some such have been available in the past, but these are, from their incompleteness, unsatisfactory now, especially as regards new materials and altered conditions of traffic. By reference to the records and use of such tests as those for the resistance to abrasion of stone and the cementing qualities of the stone powder, is shown the value of such tests, and, at the same time, the incompleteness of the present knowledge. More complete information is stated to be desirable as well as information along other lines, such for instance as the strength of stone under compression.

The changes in traffic conditions, and the new records of the experience with both old and new materials under the changed conditions are referred to with some suggestions as to what definite information may be desirable.

The statement is made that records from experience are needed, and not conclusions from assumed theories alone. Also that the author believes it may be possible eventually to reduce many of the problems of highway engineering to a mathematical solution. The beginning of the work of collecting information, and the desirability of cooperation are mentioned.

Dr. Octave Chanute and his Work in Engineering and Aeronautics: JAMES MEANS, Boston, Mass.

An appreciative review of the achievements of Dr. Chanute. (Published in full in SCIENCE.)

Permanent Winds, their Causes and Directions: W. J. HUMPHREYS, Washington, D. C.

The temperature difference between tropical regions and those of higher latitudes establishes a barometric gradient from the warmer to the colder parts of the earth. The resulting flow of the air together with the rotation of the earth causes the air of higher latitudes to flow from west to east, and that of the equatorial regions from east to west. The opposing centrifugal forces thus set up are the cause of belts of high pressure at latitudes 30° to 35° both north and south.

These belts of high pressure are underrun at five places, two in the Pacific Ocean, two in the

Atlantic and one in the Indian, by cold ocean currents. Hence, each of these five places is the seat of a permanent high or anticyclone with its attendant permanent circular winds.

An Indicator for Determining the Efficiency of Aeroplanes or Kites: S. P. FERGUSON, Reno, Nev.

The author has devised a compact self-recording instrument for indicating continuously the angle of incidence and lateral inclination of an aeroplane or kite together with the velocity, and the lateral and vertical oscillations in the direction of the wind with reference to the flying machine upon which it is carried. This instrument is based upon the kite-meteorograph designed in 1905 by the author.

Determination of Altitudes of Aeroplanes by Triangulation: R. W. WILSON, Cambridge, Mass.

This paper contains an account of observations made by two methods to determine the maximum height reached by aeroplanes and a comparison of the results obtained at the Harvard Aviation Meet.

By one method observations were made with sextants in a fixed vertical plane which the aviator was to cross at his greatest altitude. Simultaneously observations were made by the other method in which theodolites were placed at the extremities of a base-line three miles south of the field.

Technical Education in Aeronautics: C. H. PEABODY, Boston, Mass.

Technical education must eventually be offered for aeronautical engineers; the question is whether now is the time. The phases of this question are financial support, subject matter for instruction, method of securing teachers. The writer favors training a teacher for the purpose, and the establishment of undergraduate courses of instruction now. He offers a suggested course parallel to a course for naval architects.

A Program of Aeronautical Research Work, with Special Reference to what may be done at the Colleges: A. A. MERRILL, Boston, Mass.

In this paper the work suggests four general lines along which research could be profitably conducted: (1) the problem of construction, (2) the problem of efficiency, (3) the problem of power, (4) the problem of stability.

In connection with each problem the author pointed out just what research work is necessary and to what extent the colleges are fitted to undertake this work.

Some Experiments on the Pressure of a Current of Air on Certain Surfaces: G. LANZA, Boston, Mass.

The paper explains the need of an apparatus consisting of a blower, for the production of a current of air, and of a tube for directing this current against the surface to be experimented upon. Also the means of obtaining a current free from gusts, and having a uniform velocity at all points of the cross-section of the tube.

Reasons are also given why the experimental surface should not be placed inside the tube, but should be located outside of and near its mouth.

The value of K in the formula $P = KV^2$ was found as a result of these experiments to be $K = 0.0031$, whence $P =$ pressure in pounds, and $V =$ velocity in miles per hour, the experimental surface being a plane surface one foot square, placed at right angles to the current.

The results are also given which were obtained by an investigation of the vortex formed under the prow of a surface formed to approximate the underside of the wings of certain birds, and placed at small angles of inclination to the current. The need of a larger apparatus of the same kind is urged.

The Increase of Wind-pressure on a Normal Surface with Height: A. H. PALMER, Hyde Park, Mass.

The increase of wind-pressure on a normal surface with increasing height is of considerable importance in aeronautical construction. From the known decrease of atmospheric pressure with height, and the wind-velocity in the free air obtained by means of kites and observations of clouds at Blue Hill Observatory, the wind-pressure in pounds per square foot upon a vertical plane has been computed and has been plotted in a diagram.

Normal Stress and Resultant Pressure: A. F. ZAHM, Washington, D. C.

Extract from forthcoming book on "Aerodynamics" by Mr. Zahm.

Early Attempts to Navigate the Air: J. J. GREEN, Notre Dame, Ind.

Outline of history of aerial navigation from the time of Daedalus and Icarus through the middle ages down to the invention of the balloon. Scientific work done with the balloon. Its use by the postal authorities during the siege of Paris. Langley, Lillienthal, Chanute, Ader and their work; brief statements except in case of Ader's work.

G. W. BISSELL,
Secretary

EAST LANSING, MICH.